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How it means is not what it means: a defence fo Stroud's "Wittgenstein and logical necessity"

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How It Means is Not What It Means

A Defence of Stroud's "Wittgenstein and Logical Necessity"

A thesis submitted in fulfilment of the
requirements for the award of the degree

Honours Master of Arts by Research

from

University of Wollongong

by

**Luke A. Thompson. B. A. Hons.
Philosophy Program.**

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List of Abbreviations

BB	<i>The Blue and Brown Books</i> , 1958
PI	<i>Philosophical Investigations</i> , 1968
OC	<i>On Certainty</i> , 1975
RFM	<i>Remarks on the Foundations of Mathematics</i> , 1978

Abstract.

The primary task of this thesis is to provide an elaboration on and qualified defence of Barry Stroud's exposition of Ludwig Wittgenstein's ideas in his paper "Wittgenstein and Logical Necessity". This paper is Stroud's response to Michael Dummett's well known explication of Wittgenstein's views on logical and mathematical necessity in his paper "Wittgenstein's Philosophy of Mathematics". I track the debate between Dummett, Stroud, and Hilary Putnam as to the actual nature of Wittgenstein's views, but also attempt to properly situate the field of debate. I argue that the debate properly belongs to the philosophy of meaning, and on that basic claim that what Stroud has to say about Wittgenstein's targets, strategy of argument, and methodology, is correct but stands in need of some elaboration.

In elaborating Stroud's view that Wittgenstein was neither a radical nor a standard conventionalist about logical and mathematical necessity, I develop three of his claims.

(1) That Wittgenstein's main target was Platonism, but his examples of deviant rule-following undermine appeals to a notion of understanding common to both Platonist and conventionalist accounts. (2) That the examples also show that radical conventionalism is incoherent—and so not a viable alternative—by dint of their ultimate unintelligibility. (3) That Wittgenstein's critique of Platonism and conventionalism does not amount to a denial of the objectivity of mathematical and logical truth.

I also consider a claim of Putnam's, against Stroud, that Wittgenstein's view does not appear to threaten the objectivity of mathematical and logical truth because it ultimately amounts to a form of extreme constructivism. I argue that Wittgenstein explicitly rejects this view, and argue that his appeal to actual practice involving rules and agreement in form of life—agreement in using language to communicate—shows that the meanings of mathematical and logical truths are properly described as objective *and* conventional. Like Yemima Ben-Menahem, I ascribe a descriptive conventionalism to Wittgenstein, concluding that conventionalism is properly a view about meaning which does not, and cannot explain logical necessity, but only describe it.

Acknowledgments and Dedication

I would like to acknowledge the following people. Dr. John Burgess and Dr. David Simpson for their guidance in the preparation of this thesis, Talith Jennison, and my parents, Harold and Margaret Thompson, for their unfailing support, and the members of the Philosophy Program for their kind encouragement. This thesis is dedicated to my late grandmother Beatrice Joan Lockley.

Introduction

One cannot guess how a word functions. One has to *look at* its use and learn from that.

But the difficulty is to remove the prejudice which stands in the way of doing this. And it is not a *stupid* prejudice. (PI §340)

The above quote from *Philosophical Investigations* seems to me to capture an aspect of Wittgenstein's thought that is not often dealt with by his commentators. Wittgenstein seems to have been aware in his later work of how far he was departing from what had previously passed for "philosophical investigation". And he seems to have been painfully aware of the ease with which he might have been read as simply turning his back on the analytic philosophical tradition. Yet at the same time he seems passionately concerned to show us that it is all too easy to go awry in our thinking about things by falling into easily avoidable bad habits of thought. Many of these bad habits involve succumbing to the temptation to let our imagination run away with us. Though it might seem hard to imagine that a hard-headed philosophical analyst could succumb to such temptation, it is in fact all too easy. Think of the ease with which in talking about things in general philosophers fall into the habit of talking about the-thing-itself aside from what we might know, feel, see, about it. This might be the beginning of a rigorous analysis, but it is also the beginning of a flight of fancy about just how much we might be able to say about the thing in question.

In a 1998, paper entitled "Explanation and Description: Wittgenstein on Convention", Yemima Ben-Menahem, raised the issues considered in this thesis. In that paper, Ben-Menahem set out to solve the following puzzle about the philosophy of the later Wittgenstein.

Is Wittgenstein's later philosophy a kind of conventionalism? The problem is not whether Wittgenstein's ideas on the nature of logical and mathematical truth fit a certain label, that is, whether they agree or conflict with the view usually referred to as conventionalism. Rather, the problem is that

Wittgenstein's later philosophy confronts us with a paradox: it seems to both explicitly affirm conventionalism *and* persistently attack it. (Ben-Menahem 1998 p. 99)

I believe that the solution to Ben-Menahem's problem lies in the quote from *Philosophical Investigations* cited earlier. It is in the difficulty of showing that the prejudice we have about logical and mathematical truth is not a stupid one, despite the fact that it stands in the way of our coming to have a clear idea of what logical and mathematical truths are for us. We need to "look" at how we use the words that express mathematical and logical truths, but to do this we need to get away from the idea that the primary function of words is to stand for things. Despite this, we cannot allow these words to lose their ordinary sense for us, for if they do our investigations will have achieved nothing. It is because Wittgenstein is trying to do this that his later philosophy seems to "explicitly affirm conventionalism *and* persistently attack it".

It seems to me that the point brought to light here, as it relates specifically to mathematical and logical truth, is as follows: we *cannot* give any sort of adequate account of what it is to follow the rule without *mentioning* the sense of intimation at its weakest, and compulsion at its strongest, that the rule *apparently exerts upon us*. Any account of following rules that leaves this out (and by that I mean leaves it out in so far as it does not involve us describing or explaining what it is to follow the rule by saying that we seem so "intimated to" or "compelled") is inadequate to the extent that to omit this is to omit what following a rule *is for us*. Nothing stands in the way of giving an account of what it is to follow the rule which omits this aspect, but that would not be to give an account which was "philosophical" in the appropriate Wittgensteinian sense spelled out above (in that the account given preserves the sense that the words which are used to give it have in ordinary usage) though perhaps the word "post-philosophical" or "non-philosophical" would be more appropriate given Wittgenstein's own prejudices. An account that left out this feature would be a sort of anthropological or sociological account of what it is to follow rules; and by leaving this feature out I mean that the account does not say

anywhere that *we from our point of view are properly described as* “compelled” by the rule to follow it, or “intimated to” by it.

The point of this thesis is to show how Wittgenstein tries to offer an account of logical and mathematical truth that at one and the same time avoids unnecessary and undisciplined flights of imagination, *and* preserves the sense that the words used to give his account have in ordinary language. Essentially the issue I address is the same as the one that Ben-Menahem addresses in her paper. The difference between our accounts lies primarily in two areas; the larger context of our ideas on this subject, and the point of departure taken by each of us from the debate carried on about this subject in a series of papers stretching from the 1960’s through to the 1990’s. Ben-Menahem’s point of departure is the putative distinction between trivial and non-trivial conventions raised by Michael Dummett in reply to the paper by Hilary Putnam which is the last in this series that I consider (Dummett 1993 pp. 446-53). As such, her ideas, though about meaning, are framed in the context of metaphysical discourse, which is where the arena of debate shifted to after the Putnam/Dummett exchange. My point of departure is earlier in the series, starting from Stroud’s response to Dummett, and I attempt to keep the debate solely within the theory of meaning, which is where I understand Stroud to situate it.

This series of papers begins with a paper by Dummett called “Wittgenstein’s Philosophy of Mathematics”. In that paper Dummett drew out an account of Wittgenstein’s views about mathematical and logical truth from some generally accepted ideas about his view about meaning in general. Despite the fact that Dummett takes himself to be writing about Wittgenstein’s philosophy of mathematics, most of what he has to say seems to be about how mathematical and logical truths get their meaning according to Wittgenstein. On the basis of such considerations Dummett has suggested that Wittgenstein was a radical conventionalist about logical necessity, and has argued that this position is incoherent.

The second paper in the series is a response to Dummett by Barry Stroud entitled “Wittgenstein and Logical Necessity”. Stroud claims that Wittgenstein was not in fact a radical conventionalist, and that the impression that he was stems from the failure to properly understand his deployment of some crucial counter-examples to standard philosophical accounts of logical and mathematical necessity. In particular Stroud holds that Wittgenstein crafted these counter-examples with the particular aim of challenging Platonist accounts of logical and mathematical necessity. As it happens the point at which these counter-examples bite happens to be a point that both Platonist and conventionalist accounts of mathematical and logical necessity agree upon: that there is something in particular in which one’s understanding of a mathematical or logical truth consists. Stroud thinks that Wittgenstein’s rule-following considerations challenge both of these accounts by undermining the traditional philosophical notion of understanding that figures in such accounts.

The third paper in the series, and the last we consider in depth, is by Putnam, and is entitled “Analyticity and Apriority: Beyond Wittgenstein and Quine”. In this paper Putnam claims that Stroud’s response to Dummett does not address his concern head on, but revises it. And then goes on to suggest that if either Dummett or Stroud are correct about Wittgenstein then Wittgenstein must be wrong in what he thought. Putnam’s reason for holding to the later point is that he thinks that Stroud’s alternative explanation for Wittgenstein of the source of logical compulsion—our form of life—commits Wittgenstein to an altogether too radical constructivism.

Like Ben-Menahem I endeavor mainly to address Hilary Putnam in what I have to say, but also to follow up on a methodological suggestion put forward by Barry Stroud. This suggestion is basically that we need to embellish Wittgenstein’s counter-examples to the point at which it becomes obvious that they are only intended to undermine the established philosophical accounts without proposing an alternative account to replace them with. This suggestion seems in line with the generally accepted point that Wittgenstein was

concerned to avoid giving us a theory. And it seems to mesh well with the suggestion put forward by Ben-Menahem herself that if he was any sort of conventionalist at all then he held to a descriptive conventionalism which understood ordinary language appeals to conventions or rules as being free of philosophical import.

This thesis is addressed to the dialogue that builds up between these papers by Dummett, Stroud, and Putnam. In so far as the three papers I analyze are concerned I have tried to address them chronologically, and in logical sequence. I have sought to follow the author's own lines of argument as far as I can before moving onto my own commentary upon, and critique of what they have to say. That is, I go from the earliest to the latest paper, and in my treatment of each from beginning towards end. I have sought to explore a line of reply to both Dummett and Putnam on Stroud's behalf. And in doing this to reveal what I think are some neglected issues, and an alternative route to conclusions similar Ben-Menahem's own. To this end I will measure my own success in this thesis by five main indicators, which might also be regarded as the five main points this thesis sets out to make.

Firstly, I am going to argue that the issues in the debate should be considered as more properly belonging to the philosophy of meaning than the philosophy of mathematics or logic, because their primary concern is with the objectivity of the meaning of logical and mathematical truths.

Secondly, I am going to argue that the analysis that Stroud gives of Wittgenstein's ideas on logical and mathematical necessity is correct so far as it goes, and that the methodology he recommends for approaching the counter-examples—generally referred to in what follows as “Wittgenstein's examples”—is appropriate because it meshes well with much of the rest of Wittgenstein's later philosophy. In terms of his methodology, and in terms of what he was apparently committed to.

Thirdly, I am going to argue that Wittgenstein was not a radical conventionalist, based upon what Stroud has to say in his defense, and upon the incompatibility of radical conventionalism with Wittgenstein's other views, as well as the incoherence of that position.

Fourth, I am going to argue that Wittgenstein was also not an extreme constructivist, and that the grounds Putnam takes himself to have adduced for this identification are not adequate to show that Wittgenstein did in fact subscribe to extreme constructivism. I will argue that the fact that this discussion of the counter-examples more properly belongs to the philosophy of meaning shows that Putnam's attribution of extreme constructivism to Wittgenstein, if it is not irrelevant, provides support for the idea that he was not in fact a radical conventionalist anyway.

Fifth, and finally, I will endorse Ben-Menahem's ascription of a descriptive conventionalism to Wittgenstein, and to show that such an ascription is consistent with what Stroud has to say about how we should read the counter-examples, both with regard to what such a reading shows—that Wittgenstein was not a radical conventionalist—and with regard to how thus reading them sheds light on other parts of Wittgenstein's philosophy.

Finally, a word upon the motivation behind this approach. The reasons why I have adopted the outlined approach to this thesis could be seen as correlates to a set of observations made about Wittgenstein scholarship by Colin McGinn in his "Introduction" to his *Wittgenstein on Meaning: An Interpretation and Evaluation*. Four main issues can be drawn from what McGinn has to say in his introduction, and we can refer to them under the following heads: meaning, obscurantism, insensitivity, and historicity.

McGinn points out that Wittgenstein's central concern in his work, both early and late, is to foreground the primacy of considerations about meaning in philosophy generally. Though this shift is recognized by many—many people say that Wittgenstein was

primarily concerned with meaning—what it properly implies is understood by fewer: it implies that we can make no ground what so ever in philosophy until *all* issues about meaning are cleared up, until this is done we cannot even be sure that it is philosophy that we are doing anyway. Most people would not agree with such an extreme view today, but more will be said on this when we return to the issue of historicity.

Obscurantism is Wittgenstein's bane, but he could hardly have blamed it on anyone but himself. He writes with such lucidity on so many points that it is easy to overlook a very important fact about his work, as McGinn says "Wittgenstein does not always tell us his *reasons* for maintaining what he does", and it is this which allows obscurantist interpretations of his work to get a foothold: his own courting of obscurity ends in a marriage to it. (McGinn 1984 pp. xiv) We cannot, and need not, necessarily regard Wittgenstein's courting of obscurity as a *bad* thing, but to be clear on what he did have to say, we need to acknowledge, as some have, that his philosophy practically needs its own style of reading, and we need to be *sensitive* to the actual texts he presents us with.¹

Sensitivity to the actual text, and to the presentation of the text, is the next issue raised by McGinn. I agree with his general remark that "[t]oo often...expositors of Wittgenstein allow their fancies to carry them away from what Wittgenstein actually says" (McGinn 1984 pp. xiii). Unfortunate as this may be, I do not always think it is as avoidable as McGinn makes it out to be; he has already told us that sometimes Wittgenstein does not give us his reasons, and this can be very confronting. One of the few Wittgenstein expositors who to my mind clearly understands this is Michael Dummett, and I have found in much of his work a clear, and sometimes uncompromising, exposition of Wittgenstein's work, but even Dummett misses something. In some cases Wittgenstein does not give us his reasons *for* good reasons, one such reason might be that—particularly according to the view he himself is suggesting—there cannot properly be any reasons for grammatical reasons: reasons can't be given here because giving reasons is

¹ For this see Wisdom 1968 and Bambrough 1974.

against the rules (of grammar). Despite this, there are genuine examples of what we might call Wittgenstein's "capriciousness" in his work. Whether these involved him in having to give reasons he thought ought to have been obvious, or because he was just too concerned to get his point across rather than to give us reason to accept it, or because at the time of writing these reasons were too difficult to elucidate in the then predominant framework of philosophical debate. Philosophy has been changed by Wittgenstein's writings, and so we are apt to forget their historicity.

Historicity is the last issue McGinn foregrounds, and I it is an important issue for me. Wittgenstein was not writing in the contemporary setting, much less was known scientifically for example, so he often says things we would not agree with now. (See OC §286 for a good example.) But again, the whole atmosphere of philosophical debate, as well as the state of progress of various now defunct philosophical research programmes, was different. So, when I decided to write about Wittgenstein, it seemed to me that the best way to approach him was by a sort of time-travel, I would go back, situate myself in a debate very much closer to his time, and attempt from there to build up a clear picture of what he might be trying to say for myself. Wittgenstein died in 1951, and Michael Dummett's paper which forms the first one in the series I consider was published eight years later in 1959. (It is worth remarking though, that as recently as 1992, Dummett was prepared to reaffirm what he said in the 1959 paper (Dummett 1993 pp. 446-61).) I believe I have gone as far back as I can, for I am myself located in a different historical era and am therefore that much at a distance from the times I have sought to write about. It seems to me that among the authors I examine it is only Stroud who truly has a sense of the historicity of Wittgenstein's work. Stroud has recognized, correctly I believe, who the real targets of Wittgenstein's writings were, and this I believe sheds much light on Wittgenstein's own ideas. Hopefully my attempt to be similarly historically sensitive to the work of the authors I consider here will also bring some illumination, both to what was said about Wittgenstein's ideas and to what he had to say himself.

Chapter 1

Conventionalism.

§ 1. Moderate Conventionalism.

There seem to be two sorts of conventionalism, a moderate form and a more radical form. Dummett attributes the more radical form to Wittgenstein. To begin with we can say that in general conventionalists of any sort will hold that we laid down that at least some necessary statements are necessary as a matter of convention.² Dummett calls such necessary statements "direct registers of conventions". More moderate conventionalists also hold that some necessary statements will be necessary by virtue of being the "remote consequences" of direct registers of conventions. In other words: some statements are necessary because we lay down as a matter of convention that they be held to be necessary, other statements are necessary because—given certain forms of inference—we can derive that they are necessary from those statements that we hold to be necessary as a matter of convention. The difference between moderate and radical conventionalism is about the necessity of those necessary statements that are remote consequences of statements considered necessary by convention. And I take it that at least part of what is in question is whether the forms of inference are themselves to be understood as wholly conventional or not. But I think we shall also see that the very nature of convention itself is in question here.

The moderate conventionalist holds that those necessary statements which are remote consequences of direct registers of conventions are themselves logically necessary, that is, their necessity stems from the direct registers of conventions plus our forms of inference. If we have these, then in some sense, we have the remote consequences also. When describing conventionalism in general Dummett says the following about it:

² Although this way of putting it is awkward it does seem to capture something important. We say that we "laid down" that some statements are necessary in an attempt to capture the sense in which agreements about how to use certain forms of words themselves form a basis for the derivation of logical truths. The "we" here is of course not us, it is meant to refer to human beings in general, or to human society, or perhaps most specifically the community of language users.

According to conventionalism, all necessity is imposed by us not on reality, but upon our language; a statement is necessary by virtue of our having chosen not to count anything as falsifying it. Our recognition of logical necessity thus becomes a particular case of our knowledge of our own intentions. (Dummett 1968 p. 424)

Dummett gives the following example:

On this view, although all necessity derives from linguistic conventions that we have adopted, the derivation is not always direct. Some necessary statements are straight forwardly registers of conventions we have laid down; others are more or less remote *consequences* of conventions. Thus "Nothing can at the same time be green and blue all over" is a direct register of a convention, since there is nothing in the ostensive training we give in the use of color-words which shows that we are not to call something on the borderline between green and blue "both green and blue". "Nothing can be both green and red," on the other hand, is necessary in consequence of the meanings of "green" and "red" as shown in the ostensive training. We did not need to adopt a special convention excluding the expression "both green and red" from our language, since the use by someone of this expression would already show that he had not learned what he was supposed to have learned from the ostensive training. (Dummett 1968 pp. 424-5)

I think that Dummett is right when he says of the use of "both green and red" that "the use by someone of this expression would already show that he had not learned what he was supposed to have learned from the ostensive training". But what exactly did the person who underwent the ostensive training actually learn? It cannot just have been to correlate some particular range of sensations, or whatever, with the utterance of some colour-word. He must have learned something else as well, but what?

Dummett, so far has only given us the following: it is as a result of how our ostensive training with colour-words is given that "Nothing can at the same time be green and blue all over" is a direct register of convention, and that "Nothing can at the same time be green and red all over" is not. Because in our ostensive training with the colour-words "green" and "blue" we may—and often do as a matter of fact—come across borderline cases

between green and blue, we are also taught that in general we should distinguish between these two colours in the way that "Nothing can at the same time be green and blue all over" instructs us to. That is, we will probably be taught this register of convention explicitly because we *are* likely to encounter borderline cases between green and blue. Or at least situations in which equally many persons with the same ostensive training we have been given will call whatever it is "blue" as will call it "green".

On the other hand, because in our ostensive training with the colour-words "green" and "red" we are unlikely to—indeed it seems we never do—come across borderline cases between green and red, we are not also usually taught that in general we need distinguish between green and red along the lines of "Nothing can at the same time be green and red all over". So this register of convention will be implicit in what we have already been taught in ostensive training about the use of "green" and "red".

Now, Dummett does not mean by this that there are no borderline cases between green and red out there in the world, as a realist would say. That misses the point. Dummett is talking about the necessity we impose on language—and perhaps the necessity that language thereby imposes on us—not of any necessity that reality supposedly imposes on us. And this is why the first statement is a direct register of the convention and the second is an indirect one, and therefore the first is generally made explicit in our ostensive training with colour-words, while the second is generally not.

Of course, this still does not really give us an answer to a question like the following; *why* is the first statement generally made explicit while the second is usually not? If we are, as Dummett says, imposing necessity upon language, why do we do it this way and not the other way round? What makes "Nothing...green and red..." so special that we do not have to make it explicit in the ostensive training with colour-words that it is a register of the above convention? Well, there is supposedly something about the way we were taught to use "red" and "green" that makes it the case that we don't actually *have* to be taught that "Nothing...green and red...". In other words we don't actually have to be

taught to go from the first two to the third, we can get there ourselves. Yet we might still be tempted to ask how this comes about. In other words, it seems that understanding the conventions about "red" and "green", involves more than just knowing what sorts of things to call "red" and "green", it involves knowing that "red" and "green" are mutually exclusive descriptions of objects. It seems we would have to know this extra fact, since otherwise, it would seem that we *must* be taught that "Nothing...green and red..." *as well as* "green" and "red". And so we return to the question of what the person who learned how to use "red" and "green" must have learned in order to go on from "red" and "green" to "Nothing...green and red...".³

§ 2. Radical Conventionalism.

A radical conventionalist holds that *all* necessary statements are direct registers of conventions. Their necessity stems not from other direct registers of conventions and our forms of inference, but from our *decision* to treat them as necessary statements, that is, whether or not we are going to regard their necessity as stemming from direct registers of conventions plus our forms of inference. And, as I said, this seems to indicate that the forms of inference are themselves the main subject of disagreement between moderate and radical conventionalists. Unlike the moderate conventionalist, a radical conventionalist seems not to agree that we acquire an understanding of inference as part of our acquisition of language itself. Indeed, for a radical conventionalist there seem to be no forms of inference involved prior to our accepting some statement as logically necessary, just decisions about what will and what will not count as necessary statements. The transition from some set of conventionally necessary statements to a logically necessary one only seems to be a matter of inferring once we have put that logically necessary statement "in the archives" (Dummett 1966 p. 427).

³ This should also make it clear that it is not a question of what teaching is, or whether we were *actually* ever taught "Nothing...green and red...", it is rather a question of whether we *must* have been taught—in some way—that "Nothing...green and red...": and it is for this reason that empirical studies of early childhood learning are not relevant here. The question we are addressing is a logical not an empirical one; its about how the concepts fit together.

Dummett gives us an example which makes this clear involving the connection between counting and addition. We are to suppose that there are seven girls and five boys in a room. We are also to assume that we already have one criterion for deciding how many children there are in the room: counting. We can count all of the children together and that will tell us how many children there are. Alternatively, we can count first the number of girls, then the number of boys, then the number of children altogether; either way, our only criterion for judging how many children there are in the room is our use of this procedure of counting. Now, according to Dummett's Wittgenstein, when we learn to add the number of boys and the number of girls to get twelve we have in fact created a new criterion for deciding how many children there are in the room. That is, we are "adopting a new rule of language", we are adopting the rule that when we have counted seven girls and five boys in the room we can go on to assert that there are twelve children in the room (Dummett 1966 p. 426). So, our new criterion for judging that there are twelve children is: if there are seven girls and five boys, then there *will be* twelve children.

What we are to understand is that these two criteria are "distinct", that is, each can apparently stand on its own, without dependence upon the other. In Dummett's own words "if we have genuinely distinct criteria for the one statement then they may clash" (Dummett 1968 p. 426). What this means is that it ought to be possible for the two criteria to be in conflict if they are genuinely distinct, in other words, the fact that the two yield the same result is a consequence of our deciding that this is to be so. Yet, according to Dummett's Wittgenstein, "the necessity of ' $5 + 7 = 12$ '" consists just in the fact that we do not count anything as a clash; if we count all the children together and get eleven, we say, 'We must have miscounted'" (Dummett 1968 p. 426). Which apparently comes to the same as saying that ordinarily the two different criteria have been archived, and that this is why we say "we must have miscounted", and not because the criteria are actually related in any way.

What this account leaves out according to Dummett is not just the phenomenology of logical compulsion, but logical compulsion as a phenomenon. According to him, "it appears that the mathematical proof drives us along willy-nilly" (Dummett 1968 p. 426). That is, his concern is not just that Wittgenstein's account leaves out how it feels to do a mathematical proof, or in this case how it feels to understand that the two criteria (counting and addition) are interdependent, it also leaves out the fact that it seems that we cannot avoid coming to understand this very fact. It would seem that just by applying both criteria that we are in some way *almost forced* to recognize their interdependence. But it is at this point, says Dummett, that Wittgenstein "brings in some considerations about rules presented in the *Investigations* and elsewhere" to challenge the idea that the proof is capable of driving us along willy-nilly, or in this case the apparent inevitability of our coming to see the interdependence of the two criteria for deciding how many children there are (Dummett 1968 p. 426).

We will look at the "considerations about rules" in more detail later, but for the moment we can crudely summarize them as follows. There was no fact about us before we came to adopt the new criterion for deciding how many children there are in virtue of which we can say that we would have come to accept *just that* criterion. Our acceptance of *that particular criterion* for deciding how many children there are in the room did not require that there be any particular thing being said, done, or thought by us before we accepted it. This is because the rules that tell us how to make the inferences from each step of the proof to the next have to be interpreted, and there will be some interpretation of *those rules* on which we would not proceed in the way that we are accustomed to, and yet this interpretation of those rules would be consistent with how we have followed those rules up until now. Even if we try to formulate rules for interpreting these rules of inference, they themselves will also be open to an interpretation which would have us proceed in a different way to one in which we are accustomed. As Dummett describes it:

But in order to follow the proof we have to recognize various transitions as applications of the general rules of inference. Now even if these rules had

been explicitly formulated at the start, and we had given our assent to them, our doing so would not itself constitute recognition of each transition as a correct application of the rules. Once we have accepted proof, we shall indeed say that anyone who does not accept it either cannot really have understood or cannot really have accepted the rules of inference; but it does not have to be the case that there was anything in what he said or did before he rejected the proof which revealed such a misunderstanding or rejection of the rules of inference. (Dummett 1966 p. 426-7)

Now, Dummett moves immediately from the quote above to the one which follows, but I want the reader to note something about what has thus far been said. Dummett has stated, I believe correctly, that there need not have been anything anyone who rejects the proof "said or did before he rejected the proof which revealed such a misunderstanding or rejection of the rules of inference". What we should note is that before the proof was accepted, we and he were in this same situation. But the crucial difference, is that when we have accepted the proof and he has not, then apparently he has misunderstood or revoked. The crucial question here is about what is it that we have done that he has not. This is important. We moved on as a group; he was left behind by himself. How we moved on as a group given that nothing in what we said prior to moving on indicated what was to actually count as moving on, at this point, remains a mystery. Now, as I said Dummett continues this passage as follows.

Hence at each step we are free to choose to accept or reject the proof; there is nothing in our formulation of the axioms and rules of inference, and nothing in our minds when we accepted these before the proof was given, which of itself shows whether we shall accept the proof or not; and hence there is nothing which *forces* us to accept the proof. If we accept the proof, we confer necessity on the theorem proved; we "put it in the archives" and will count nothing as telling against it. In doing this we are making a new decision, and not merely making explicit a decision we had already made implicitly. (Dummett 1968 p. 427)

So what Dummett means by saying that Wittgenstein was a radical conventionalist is that Wittgenstein apparently believes that it is possible for us to make alternative

interpretations of the rules of inference, and hence that strictly speaking, we *decide upon rather than infer* the conclusion of a proof, or even its individual steps. But what we also ought to remember here, is that this is apparently *our* decision, not just mine or yours. This is important because Dummett goes on to give a brief account of Wittgenstein's most famous example of this type, the one involving a pupil taught to continue a series by recursive addition, and then asks the following question.

But suppose the training was not given only by example, but made use also of an explicit formulation of the rule for forming from an Arabic numeral its successor. A machine can follow this rule; whence does a human being gain a freedom of choice in this matter that a machine does not possess? (Dummett 1966 p. 428)

Most readers of Wittgenstein will have anticipated the answer to this question; that what Dummett is talking about here is a rule for interpreting a general rule of inference and that this further rule will necessarily suffer the same fate as the first. Dummett of course recognizes this but he says that "such considerations seem to belong to the theory of meaning in general, rather than having any particular relevance to the philosophy of mathematics" (Dummett 1966 p. 428). According to him we ought to read Wittgenstein here as claiming, as the intuitionists did, that we cannot demarcate in advance that some forms of argument are the only acceptable lines of appeal in doing mathematics.

Dummett presents a final example to illustrate what he takes to be Wittgenstein's view. We are to imagine that there somewhere exists a document with a proof for a certain statement of mathematical theory, and that the theory of which this statement is part is itself complete, i.e. everything the theory asserts is provable within the theory itself. We are also to imagine that this document has not yet been seen by human eyes. According to Dummett's Wittgenstein, the proof on the document does not actually constitute a proof within the theory until we have discovered and accepted it as a proof within the theory. In other words, even when the proof is discovered we are still free to decide whether it counts as a proof within the theory or not. Dummett goes on to say that this "conception

is extremely hard to swallow, even though it is not clear what one wishes to oppose to it" (Dummett 1966 p. 429). And it seems that what he means by this is that although we might not want to say with the Platonist that the proof did definitely exist as a proof within the theory prior to our discovery of it, we might be equally hesitant to say that its status as a proof in the theory is dependent upon us so conferring it after the proof has been discovered. We should remember here that the proof *was already written down* so we are assuming it existed prior to its discovery, what we are in a quandary about is whether or not it existed *as a proof within the theory* prior to its discovery. This is important because the point being raised here is not the typical one raised when Platonists are opposed to non-Platonists in a discussion of mathematical proof. The point is normally one about the existence of the proof at all. But in this case that has been assumed. So the point under consideration is one about what it means to say that a certain proof is part of a certain theory, and not about what it is for there to be or not to be such a proof. It's not that the proof necessarily exists, it's that it is necessarily part of the theory given that it does.

These last two points are to my mind contentious ones. It is not clear to me that the question about whether or not something counts as a proof in a theory given that it already exists, but has yet to be discovered, actually belongs only to the philosophy of mathematics. It seems to me that *this is* a question that belongs to the theory of meaning generally. Were this a question about whether the proof exists in the first place or not, then I would concede Dummett his point here, *but this is a question about the meaning of the proof given that it does exist*, and that seems like a question for the theory of meaning to me.

Again the point that immediately precedes the above one, about the machine's lack of freedom in following the rule. To say that the machine lacks the freedom to deviate from the rule is itself to work with a particular conception of the machine, in particular one that conceives of the machine as necessarily following the rule by its very nature. Wittgenstein

himself described this as "the machine as symbolizing its action" (PI §193). Of this conception of the machine Wittgenstein says that:

We say, for example, that a machine *has* (possesses) such-and-such possibilities of movement; we speak of the ideally rigid machine which *can* only move in such-and-such a way.—What is this *possibility* of movement? It is not the *movement*, but it does not seem to be the mere physical conditions for moving either—as, that there is play between socket and pin, the pin not fitting too tight in the socket. For while this is the empirical condition for movement, one could also imagine it be otherwise. The possibility of movement is, rather, supposed to be like a shadow of the movement itself. But do you know of such a shadow? And by a shadow I do not mean some picture of the movement—for such a picture would not have to be a picture of just *this* movement. But the possibility of this movement must be the possibility of just this movement. (See how high the seas of language run here!) (PI §194)

The important thing to remember here is that the very process which Dummett seeks to use the machine analogy to appeal to, is required for the understanding of the analogy that he is relying upon in order to pose his question. It is part of our imagining the machine as "ideally rigid" that we do not allow that the "possibilities of movement" are anything other than what that conception of the machine requires; they are in fact part of the conception itself. To specify them independently, to see each as a "picture of the movement" is to change the conception of the machine. We now work with a different symbol, one with separable parts, whereas our first had none. That is why the picture of the possibility of movement "would not have to be a picture of just *this* movement." A picture cannot be the same as what it pictures, so language with its pictorial bias fails Wittgenstein here, and it seems that all that can be said is that "the possibility of this movement must be the possibility of just this movement". We could perhaps rephrase this. This possibility of movement will not be the possibility of just this movement if it is anything other than that original possibility, but for it not to be anything else, we cannot speak of it pictorially,

historically, or separately: it is one and the same as the conception of the machine of whose movement it is a possibility. (PI §§193-4)

Lastly, we should note that Dummett thinks that examination of Wittgenstein's own examples will show that the radical conventionalist position that he ascribes to him is an implausible one.

The examples given in Wittgenstein's book are—amazingly for him—thin and unconvincing. I think that this is a fairly sure sign that there is something wrong with Wittgenstein's account. (Dummett 1966 p. 430)

This point will be more relevant later on, but it is worth our while to observe that although Dummett has made this assertion, he has not made extensive reference to, or examination of, the very examples that he thinks do show Wittgenstein was a radical conventionalist, and that that view is a flawed one.

I think we can sum up Dummett's conception of Wittgenstein's view with the following quotes drawn from the part of his paper in which he most explicitly contrasts that view with the views of Platonists and intuitionists.

He [Wittgenstein] appears to hold that it is up to us to decide to regard any statement we happen to pick on as holding necessarily, if we choose to do so. The idea behind this appears to be that, by laying down that something is to be regarded as holding necessarily, we thereby in part determine the sense of the words it contains; since we have the right to attach what sense we choose to the words we employ, we have the right to lay down as necessary any statement we choose to regard as such. Against this one would like to say that the senses of the words in the statement may have already been fully determined, so that there is no room for any further determination....Wittgenstein's...idea, that one has the right simply to *lay down* that the assertion of a statement of a given form is to be regarded as always justified, without regard to the use that has already been given to the words contained in that statement, seems to me mistaken. If Wittgenstein were right, it appears to me that communication would be in constant danger of simply breaking down. (Dummett 1966 p. 433-5)

We should note here a few things about the view attributed to Wittgenstein; firstly, the view is spelled out in terms of statements and necessary statements, and not specifically in terms of proof. While this is not required for the thesis of radical conventionalism, it is interesting that Dummett did say earlier that he thought that considerations advanced in favor of Wittgenstein's case, according to him, were not as relevant because those considerations were not about proof and so belonged to the theory of meaning rather than to the philosophy of mathematics. Secondly, the statement "we have the right to attach what sense we choose to the words we employ", is not wholly unambiguous. In one sense this can be a rather radical claim about what individuals may do with the words they employ, and in this sense it seems right to say its is obviously absurd. In another sense this can be a more moderate claim about what we as a group may do with the words we employ, and in this sense it is not clear that Wittgenstein's position is as absurd as Dummett makes it sound.

Now, we will leave aside for the moment the questions of whether or not Dummett has actually succeeded in attributing radical conventionalism to Wittgenstein, or even spelling out exactly what radical conventionalism comes to for that matter. We will move on to take a look at what seem to be the two main flaws of radical conventionalism as Dummett seems to be conceiving of it here, so that if nothing else we at least have clear idea of what radical conventionalism is from Dummett's perspective.

§ 3. Intuitions.

It should be obvious that the radical conventionalist view clashes with our most basic intuitions about how necessary and logically necessary statements are related. Our most basic intuitions about what is for something to be a necessary statement seem to be that if something is necessary then it *must* be the case, and if it is logically necessary *that it could not be the case that it was not the case*. A statement is logically necessary if it follows necessarily from some set of statements that are laid down as necessary by convention. Conventionally necessary and logically necessary statements are therefore related in just

this way; the necessity of the later stems from the necessity of the former, and given that the former are necessary it is necessarily the case that the later are necessary. Not only this, but it seems that it is not possible for us to escape so inferring, the necessity of a logically necessary statement seems to follow from its very meaning. We seem, as Dummett suggests, to run up against something very counter-intuitive when we try to think of ourselves as *choosing* whether or not to go on to infer such a statement. We seem compelled to go on in the way we customarily do. It seems immensely difficult to imagine with any clarity doing things in any other way. Can we really clearly imagine sincerely believing that $2 + 2$ comes to 5 and not 4? Don't we rather imagine this in isolation, from what we already do? Could we really accept having to pay for five beers when we ordered two for ourselves and two for a friend?

§ 4. Mistakes and Incoherence.

The second major flaw of radical conventionalism that Dummett seems interested to draw our attention to is its incoherence as a position within the philosophy of mathematics. Imagine that we have met someone who is not familiar with the concept of addition. This person has only one criterion for deciding how many children there are in the room that contains five boys and seven girls: counting. The only way for this person to be sure in judging that there are twelve children in the room is for them to count all of the children together. But this leaves open the possibility that on one occasion they may count all of the children together and judge that there are twelve children and on another that there are thirteen. We may, on meeting such a person, attempt to demonstrate to them that if there are five boys and seven girls in the room, then there *will* be twelve children; and so introduce them to another criterion for judging how many children there are. We would do this by introducing them to the concept of addition. We would introduce them to the idea that counting first five boys and seven girls, and then twelve children, comes to the same thing as adding five and seven to get twelve, or $5 + 7 = 12$.

Now, as Dummett rightly points out, it seems at this point that the person would have come to recognize a new criterion for judging that there are twelve children in the room, as Wittgenstein would say, they have learned a new rule of language. The criterion of addition has been introduced as a criterion for judging that there are twelve children in the room, and this criterion differs from their previous one, in particular it does not require that they count more than twice i.e. count seven girls, then count five boys, then add these to get twelve children. But we also expect that they will realize that the concepts of counting and addition are interdependent, that conflict between the two criteria is not acceptable. Yet this is not something made explicit when the criterion of addition is introduced, we take it that they will see this is so, but might they not notice? Or worse still, might they fail to recognize this at all, might they come to see the criteria as entirely distinct, so that they take it that if you add you will get twelve, but if you count you could end up with either twelve or thirteen? This seems impossible, or at least very counter-intuitive. As Dummett goes on to say:

If we say that if he counted five boys, seven girls, and thirteen children, then there must have been something which, if he had noticed it, he would have regarded as a criterion for having miscounted, then the effect of introducing him to the concept of addition is not to be simply described as persuading him to adopt a new criterion for having miscounted; rather, he has been induced to recognize getting additively discordant results as a *symptom* of the presence of something he already accepted as criterion for having miscounted. That is, learning about addition leads him to say, "I miscounted," in circumstances where he would not before have said it; but if, before he had learned, he had said, "I miscounted," in those circumstances, he would have been right by the criteria he then possessed. Hence the necessity for his having miscounted when he gets additively discordant results does not, as it were, get its whole being from his now recognizing such results as a criterion for having miscounted. (Dummett 1968 pp. 431-2)

Let us straighten out what Dummett says here. If he has a concept of counting then he also has concept of miscounting. If counting is his criterion for judging how many children there are in the room, and he has a concept of miscounting, then he will be able

to understand the idea that he has not employed his original criterion of counting correctly: that he has made a mistake of some kind. According to Dummett, when he learns to add, he also learns to recognize additively discordant results as a symptom of having miscounted. So when he learns to add he learns to recognize yet another thing as a symptom of miscounting, and this is supposedly lumped together with any other things he has learned to recognize as symptoms of miscounting. Now, it seems strange to say that he has been introduced to an entirely new criterion for judging how many children are in the room when we teach him to add, if we are also teaching him to recognize yet another thing as a symptom of the misemployment of his original criterion for judging how many children there are in the room. But apparently this is what Wittgenstein would have us believe, that the criteria for adding are entirely new ones for him, that they can make no difference to when he would judge himself to have miscounted. But this just seems wrong if we keep in mind that he would already have presumably had some things that he recognized as symptoms of his misemployment of the counting criterion, and that this has just been extended upon and not changed; he still recognizes all the things that he did before as symptoms of his having miscounted. And that he therefore recognizes that if any of these symptoms are present he has made a mistake, and that this will be a particular identifiable mistake. To say that he could learn to add as we understand it and that he could somehow fail to recognize additively discordant results as a symptom of the same kind as other symptoms he recognizes of miscounting, is to say in effect that he has not learnt to add as we understand it.

Another way to see this is as follows. Before we teach him to add there will be times when he has miscounted but doesn't realize and when he would have accepted he had miscounted if he had noticed he had done so or if some symptom of miscounting had been present. These will just be unfortunate occasions when he misemploys his own criterion for judging how many children are in the room, and when there is not anything present that he recognizes as a symptom of this happening—he doesn't remember counting any of the children twice etc. Now, when he learns to add there will be

occasions of just this type where he will instead judge that he has miscounted and will admit that even though he would not have judged this way before, he would still have been supposed to judge that he had miscounted given his own criteria of miscounting. The difference is that now he has a symptom of his having miscounted in these situations. The results don't add up.

We can now see how the problem is also one about intention. If his intentions when he says "I am going to add five and seven", are anything like our own then he would have to recognize that if as a result he comes up with thirteen as his answer something has gone wrong. In particular, if we imagine him actually adding on paper right in front of our eyes—by say, placing down five dashes and on the next line seven dashes and then counting them all up by putting a number next to each in subscript—it seems almost impossible that he could not recognize an additively discordant result as a symptom of miscounting, *and* be able to recognize himself and show us, where and how it was that he went wrong. Yet according to Dummett (perhaps correctly) Wittgenstein wants to maintain that he could do something like this and that for him there would be no specifiable mistake, let alone anything he could point to to show where or how he went wrong. Indeed it seems he would not understand the idea that he had made any sort of mistake at all.

To finish off consider the following children's joke.

- (a) Did you know there are eleven fingers on your two hands?
- (b) How do you mean?
- (a) (Counting backwards from ten on the fingers of the right hand) Ten, nine, eight, seven, six, and (holding up left hand) five, is eleven!

In this case, what momentarily takes us in is that the procedure used to count, though a bit odd, seems fine. All the original counting is in sequence, albeit backward sequence, and the addition is one we are very familiar with, and we are all aware that in general human beings have five fingers on each hand. The trick is of course that (a) swaps

calculating procedures without making the fact explicit. Yet there is not, so far as I am aware any prohibition on swapping calculating procedures when doing a mathematical sum, so long as the answer is correct, so there seems to be nothing that prevents (a) from going about the calculation in this way except that they get, according to us, the wrong result. What this example is supposed to show is how it easy it is to get mixed up in a way similar to the way in which Wittgenstein says the person to whom we are teaching addition may get mixed up. Of course if we look more closely at what is going on we notice that (a) is actually playing on the ambiguity of "six", as being, a term in a sequence and a term denoting a quantity, and this brings us back to the idea that it is only because our initial ostensive training does not make it explicit that using these two senses of "six" are mutually exclusive that the joke is in any way humorous.

Chapter 2

Wittgenstein's Targets.

§ 1. Introduction.

In this chapter and the next two we move on to consider a response to Dummett's concerns advanced by Barry Stroud. Stroud considers two types of examples in his response, and we are going to focus on his consideration of each in individual chapters. This chapter introduces and explores Stroud's claim that the true target of Wittgenstein's writing on logical necessity is in fact Platonism, and goes on to lay out in schematic form what he takes to be Wittgenstein's strategy of argument. Chapter three deals with the first type of example which typically involves an individual who deviates from the usual practice in following some rule. And chapter four involves consideration of a second type of example which involves a whole community who follow rules in a strange way. Though Wittgenstein's work actually contains more examples of the former type, it is reasonable to think, as Stroud seems to, that he was using both sorts of examples to develop a particular theme. Stroud's contention seems to amount to the claim that not enough attention has been devoted to examples of the second type, and that this has led Dummett awry in his interpretation of Wittgenstein's views on logical necessity.

§ 2. Where Dummett Left Us.

According to Michael Dummett, Wittgenstein held that the necessity of statements that are themselves inferred from statements held to be necessary by convention is a matter of decision. That is, for Wittgenstein, the fact that a statement is logically necessary is just a matter of our decision to treat that statement as unassailable, regardless of the necessity of the statements from which it has been inferred. This conclusion apparently follows from Wittgenstein's insight that the general principles of inference cannot of themselves determine how we are to apply them. Dummett went on to show that the idea that logical

necessity is a matter of decision is unacceptable, because it turns out to be both counter-intuitive and incoherent. In short, we would have to accept that by our lights someone would not be doing anything contradictory if he understood and accepted the general principles of inference, but then deviated from the usual practice in his application of them while maintaining that he was going on in the same way. The following quote provides an illustration in terms of mathematical proof.

For Wittgenstein, accepting the theorem [by following a proof] is adopting a new rule of language, and hence our concepts cannot remain unchanged at the end of the proof. But we could have rejected the proof without doing any more violence to our concepts than is done by accepting it; in rejecting it we could have remained equally faithful to the concepts with which we started out. It seems extraordinarily difficult to take this idea seriously when we think of some particular actual proof. It may of course be said that this is because we have already accepted the proof and thereby subjected our concepts to the modification which acceptance of the proof involved; but the difficulty believing Wittgenstein's account of the matter while reading the proof of some theorem with which one was not previously familiar is just as great. We want to say we do not know what it would be like for someone who, by ordinary criteria, already understood the concepts employed, to reject this proof. (Dummett 1966 p. 430)

And according to Dummett this leaves us with the following unpalatable fare.

Wittgenstein's conception is extremely hard to swallow, even though it is not clear what one wishes to oppose to it. The proof is supposed to have the effect of persuading us, inducing us, to count such-and-such a form of words as unassailably true, or to exclude such-and-such a form of words from our language. It seems quite unclear how the proof accomplishes this remarkable feat. (Dummett 1966 p. 430)

In a response to Dummett, entitled "Wittgenstein and Logical Necessity", Barry Stroud argues that Wittgenstein did not believe that a statements status as logically necessary was just a matter of our decision to treat it as unassailable (Stroud 1966 pp. 477-96). Stroud thinks that while Wittgenstein's insight does lead onto a critique of certain views about

logic, it does not imply that the unassailability of logically necessary statements is a matter of arbitrary decision. According to Stroud, Dummett has not made a careful enough examination of the examples which Wittgenstein uses to illustrate his insight and the implications that flow from it. Nevertheless Stroud does admit that Dummett has a point about the view that he, wrongly, attributes to Wittgenstein, and his claim that that view is incoherent is correct.

Dummett is obviously on strong ground here—it seems impossible to understand this alleged possibility—but I think Wittgenstein would agree. His examples are not designed to show that we do understand this. What is important for the problem of logical necessity is to explain what makes the denial of a necessary truth “impossible” or “unintelligible.” It is not enough to say that it is “logically impossible,” since an explanation of logical necessity is just what is in question. (Stroud 1966 p. 478)

Stroud therefore sees his own task as that of showing how Wittgenstein accounts for the fact that the denial of necessary truth—of the logical necessity of statements themselves inferred from necessary statements—is unintelligible in a case like the one above. According to Stroud, Wittgenstein attempts to do this by pointing out that for there to be any such things as calculating, counting, or inferring at all, it is necessary that there be general agreement about what counts as calculating, counting, and inferring correctly. Again, as Stroud says, this is just where calculation and experiment differ, with it being acceptable in the case of the latter to get different results at different times, or for different people to get different results. Stroud then says that such “remarks suggest that the source of necessity in inferring or calculating is simply that any activity in which just any results were allowed would not be called “inferring, “calculating,” and so forth” (Stroud 1966 p. 479).⁴ And he presents the following quote from Wittgenstein’s *Remarks on the Foundations of Mathematics* as a sample of Wittgenstein’s own statement of the view.

⁴There are a few things to say at this point. There are those who argue that Wittgenstein did actually mean to describe calculation as being like experiment in contrast to Stroud, and this is one line of attack or objection that some interpreters would use contra Stroud (see Bloor 1997 for instance). Secondly, it seems that Stroud might well be wrong about this difference—and perhaps Wittgenstein too—because of something called “the experimenters regress” (Collins 1985 pp. 129-30). The experimenters regress is said

The steps which are not brought into question are logical inferences. But the reason why they are not brought in question is not that they “certainly correspond to the truth”—or something of the sort—no, it is just this that is called “thinking,” “speaking,” “inferring,” “arguing”. (RFM, I, §156)

Even so, Stroud urges us to acknowledge that Dummett's critique is correct in so far as it shows that what Wittgenstein did have to say does pose problems for the standard conventionalist view. And in this he seems to echo Dummett's misgiving that "Wittgenstein's conception is extremely hard to swallow" (Dummett 1966 p. 430).

Despite suggestions of this “standard conventionalism” in Wittgenstein, I agree with Dummett that he does not hold such a view, although it is not always easy to see how what he says differs from it. (Stroud 1966 p. 479)

To see how what Wittgenstein has to say does depart from the standard conventionalist view, Stroud says that we need to attend not only to what Wittgenstein had to say, but also to who he took the targets of his own arguments to be.

§ 3. Wittgenstein's Targets.

According to Stroud the target of Wittgenstein's writing about mathematics, meaning, and necessity, is the Platonism of philosophers like Frege and Russell. So, while Wittgenstein's views can also be seen to be critical of standard conventionalism, their main target is in fact Platonism (Stroud 1966 pp. 479-89). Accordingly, we find that Wittgenstein's critique of standard conventionalism only comes about as a later development, and that when he is considering alternatives to Platonism, he proceeds to raise a few difficulties with conventionalism in general. But before we go any further we need be aware of what Stroud says here implies.

to affect the reliability of both the experimenter and the experiment. In the case of the former, competence as an experimenter depends upon achieving expected results, but what count as expected results depends upon what has counted as a competent performance of the experiment in the past and so on. In the case of the later, an experiments adequacy to show what it apparently does, to confirm hypothesis H for example, can only be established by replication of that experiment, but the adequacy of the replication to test the adequacy of the first performance of the experiment can only be tested itself by further replication, and so on.

If Stroud is correct, and the target of Wittgenstein's writings on necessity *is* the Platonism of philosophers like Frege and Russell, then a shift of focus is in order. We need to read Wittgenstein's "thin and unconvincing" examples as directed, not at Dummett's modified conventionalism, but at the more robust Platonist thesis about logical necessity (Dummett 1966 p. 430). In refocusing our attention, we are going to need to reinterpret these examples, *and* we are going to need to be aware of a few things. Firstly, we are going to need to see exactly what Wittgenstein is taking aim at in making this challenge to Platonism. Secondly, we are going to need to see what remains untouched by the critique the examples provide. I propose to treat these points in reverse order, because I believe this will leave us better able to see how Wittgenstein ends up being positioned on these issues according to Stroud.

At the end of his article Stroud says that he is "primarily concerned" to explain that what Wittgenstein did have to say about logical necessity, does not destroy the "objectivity" of the rules that give us the general principles of inference (Stroud 1966 p. 496). So according to Stroud, the objectivity of the general principles of inference, and presumably of the conclusions which are deducible using them, remains untouched by the critique the examples provide. We will return in the last chapter to provide backing and context for this point, but for the moment we will take it as given so that we might get a more perspicuous view of the challenge to Platonism. So what does this tell us?

Well, at first glance, it would seem that in order to argue successfully against the Platonist conception, Wittgenstein also has to argue against the objectivity of mathematical and logical truth. There are a couple reasons one might think this was so, both given to us by Dummett.⁵ To argue against the Platonist it seems we have two options, the first is to argue ontologically . But as Dummett points out.

⁵ These are correlates of the two main objections that he has to radical conventionalism. That it implies that mathematical and logical reasoning cannot be objective, because at each step it is up to us to decide how to go on, or what counts as going on.

As Frege showed, the nominalist objection to Platonism—that talk about “abstract entities” is unintelligible—is ill-taken; if we believe in the objectivity of mathematics, then there is no objection to our thinking in terms of mathematical objects, nor to the picture of them as already there waiting to be discovered that goes with it. (Dummett 1966 p. 423)

It would appear that for the challenge to Platonism to be successful and to be seen as more than an expression of philosophical prejudice, Wittgenstein has to argue both against the objectivity of mathematical reasoning, *and* against the idea of “thinking in terms of mathematical objects” with the “picture of them as already there”. The reasoning here is that arguing that “talk about ‘abstract entities’ is unintelligible” cannot in itself show that the Platonist is wrong. To show that, one must argue that *there is no absolute sense* in which we can be said to be going on in the *wrong* way in following a rule if we go on differently to way we usually do. What has to be shown is that there are no actual rules somehow *existing independently* of our following them.

The second reason is that even our collective intersubjectivity does not seem to provide the resources necessary to guarantee the objectivity of mathematical reasoning. This point can be put most perspicuously by re-examining the positive argument put forward in favor of radical conventionalism and against standard conventionalism, because it draws upon reasoning about the nature of convention very much like Wittgenstein's own. This view is referred to by Dummett as “full-blooded conventionalism” and we have so far referred to it, as Stroud does, as “radical conventionalism”.

The radical conventionalist thesis can perhaps be most clearly stated as follows. Since all transitions involved in reasoning from premises to conclusion make use of the general principles of inference, and since the general principles of inference do not themselves determine how we are to apply them, there is no way to tell *a priori* what would count as the conclusion that ought to follow from some particular set of premises. Given this, the fact that we have laid down that certain statements be treated as unassailable does not show *a priori* that certain other statements follow from the former ones as logically

necessary, i.e. necessary in virtue of the fact that they follow as the conclusions of inferences in which the premises are those statements we have decided to treat as unassailable. And the radical conventionalist asserts, as a positive thesis, that we therefore decide which statements are to count as logically necessary, or what to accept as the appropriate conclusions of such inferences. (It should hopefully be clear what sort of role Wittgenstein's insight about general principles of inference, or the rules of inference, would play in this account as Dummett attributes it to him.)

The main thing to take note of here is that without *something* to guarantee the objectivity of mathematical reasoning over and above mere human caprice, it seems impossible to be sure that what we call mathematical reasoning will be objective in anything other than the most shallow sense; that is, in so far as we call what we typically describe as mathematical and logical reasoning "objective". That this is a difficulty that arises *because* Platonism is being challenged, is made obvious by the fact that the same difficulty pops up when conventionalists of any sort, argue against the Platonist thesis that mathematical and logical truths exist independently of our awareness of them. As Stroud points out.

The conventionalist's opposition to Platonism consists primarily in showing that our present ways of inferring, counting, and so forth, are not the only possible ones. But the standard conventionalist would also reject the alleged possibility on the grounds that the description of such a state of affairs is contradictory. (Stroud 1966 p. 480)

The "alleged possibility" here referred to is the radical conventionalist proposal that we have the right to decide what counts as going on the same way, and so what counts as properly inferring and so forth. The difficulty of arguing against Platonism while attempting not to impugn the objectivity of mathematics arises because the proposal that "our present ways of inferring...are not the only ones" undermines any appeal to these ways of inferring to support the claim that some piece of mathematical reasoning is objective. How can a chain of reasoning be said to be objective, in anything other than the

most shallow sense—as a mere form of words—if we cannot appeal to some standard or norm to show that coming to any other conclusion is incorrect? Stroud of course suggests that for the standard conventionalist, "the description of such a state of affairs is contradictory", but what does this mean? It seems that this comes to as much as saying that situation as thus described makes no sense. But this has not yet given us mathematical objectivity: saying it makes no sense, does not tell us why it makes no sense. And so far the Platonist has one up on the conventionalist because they *can* say why, since for them the principles of inference are not constituted by convention, and so their objectivity does not depend upon how we describe the situation, but on how it actually is. The conventionalist doesn't seem to have gotten us beyond the most shallow sense of "objective"; and so they are still vulnerable to the radical conventionalist charge that no more than arbitrary decision is involved here. To get mathematical objectivity in anything other than the most shallow sense *and* successfully challenge Platonism we would need to provide an argument that supports Dummett's intuition about radical conventionalism.

What is not clear to me is that rejecting the Platonist's conception involves adopting this line about proofs; it seems to me that a man might hold that, once the proof was discovered, we had no choice but to follow it, without allowing the correctness of saying, before the proof was discovered, that either there is a proof or there is not. (Dummett 1966 p. 429)

To see what such an argument might come to, and how we might provide it we can now turn our attention to exactly what Wittgenstein is taking aim at in making this challenge to Platonism.

§ 4. Saving Objectivity to Challenge Platonism.

The objectivity of the meaning of mathematical and logical truth can be argued for in two ways; ontologically, or non-ontologically. The Platonist argues for the objective meaning of logical truths *on the basis* of an ontological claim about those truths; on the

basis of the claim that those truths exist independently of our recognition of them. Our recognition of their existence supposedly constitutes our coming to understand that their meaning is objective. We can perhaps make this clear with an example. The Platonist argues that we come to see that the meaning of say, " $2 + 2 = 4$ " is objective, when we grasp the rule for adding two, analogous to the way in which we come to see that it is an objective fact that a circle is not the same shape as a square by our discerning the difference between the two shapes. Again, we come to see of something that it is a horse by coming to see that it participates in the form of a horse, which is independent of how it might appear to us.

Customarily, this Platonic line of argument is rejected because it is said not to address the central question, which is not a question about the ontological status of mathematical and logical statements but about how we come to understand that their meanings are objective. This question is about *how we understand* mathematical statements, rather than about *what mathematical statements are like*. Now we must be cautious. In so far as the Platonist is making an argument about what counts as acceptable justification in mathematical and logical reasoning, the question about meaning *is* connected with the ontological one. To say what counts as acceptable justification is to demarcate appropriate from inappropriate reasoning in mathematics. It is in arguing for the *objective status of claims about justification* in mathematics that appeal is made to idea that the meanings of mathematical statements are objective. Hence, Dummett says above that, "it seems to me that a man might hold that, once the proof was discovered, we had no choice but to follow it" (Dummett 1966 p. 429). What this means is that the proof can command assent because the reasoning involved can be justified objectively, that is; without resort to the details of any particular individuals understanding of it. This idea relies upon the idea that the meanings of the statements that compose the proof can also be said to be objective—to fit together so as to give us the proof in question regardless of whether any individual understands them as so doing or not. So what is at issue here is the *grasp* that we have of that objective meaning of those mathematical statements. But we should remember that

without such a grasp we will be in no position to see how the proof is justified objectively—without resort to details of any particular individuals understanding of it.

A more devastating objection is the one that is usually taken to be raised by Wittgenstein himself. This objection is basically that the ontological basis of the claim makes no difference. We shall see in a later section how the rule-following considerations form the kernel of this objection, but in so far as they are only directed against Platonism, as Stroud says, the objection is essentially the one which conventionalists raise to Platonism. "The conventionalist's opposition to Platonism consists primarily in showing that our present ways of inferring, counting, and so forth, are not the only possible ones" (Stroud 1966 p. 480). Which is to say that understanding or grasping the rule involves some form of interpretation. As I have said, the basis of this objection is that the ontological claim that forms the basis of the Platonist conception does not show of itself what would constitute understanding of the rule, it does not show what it is for us to grasp the rule. If we are said to be able to discern the rule in the same way in which we can discern the difference between a circle and a square, then it seems that we should be able to show how it is that someone who did not go on as we did in following the rule has *always* gone wrong. That there must be some way to show that "our present ways of inferring, counting, and so forth" *are the only possible ones*, and *this* requires that we show what makes for a proper grasp of the rule, what makes for an understanding of it. The rule-following considerations mount a strong case to show that this is not something we can in fact show. And as Dummett's example about the proof being already written down shows, regardless of whether mathematical and logical truths are 'out there' or not, that if we cannot show what makes for a proper grasp of them, then we cannot say our understanding of them is an understanding of something objective.⁶

As Dummett suggests in the quote above from page 429 of his 1966, this is not the only way to argue for the objective meaning of mathematical and logical truths, or even of

⁶ See chapter 1, §2, for this.

mathematical and logical statements. There are, to be sure, at least a few ways in which we can argue for the objectivity of mathematical and logical truths non-ontologically, *one* of these ways involves arguing for their objectivity on the basis of *understanding*. This is the strategy favored by the standard conventionalist. The conventionalist argues that logical truths are created by conventions that stipulate rules for how one is go on in, say, inferring; but that one has only properly grasped such a rule when one comes to see that the meaning of the logical truth is objective. This in practice means coming to see that there is only one way in which the rule can be "unpacked" in application. As such, if one does not recognize the objectivity of logical truth then one is said either not to have understood or grasped the rule, or to have revoked on the convention that created it. As Hans Hahn says in his paper "Conventionalism", someone who asserts, say, that such-and-such is both red and green all over, "has violated the rules in accordance with which we want to speak, and I shall refuse to speak with him any longer" (Hahn 1968 p. 49).

We will return to the subject of conventionalism and what motivates its rejection of the Platonic metaphysics of logic and mathematics in the last chapter. What should be clear at this point is that there is more than one way to argue for the objectivity of the meanings of mathematical and logical truths, but that any *explanation* of mathematical and logical necessity, will need to appeal to some notion of understanding as primitive. This is particularly so if that explanation is also going to tell us how what counts as appropriate justification in mathematical and logical reasoning is an objective matter. Finally we should note that any explanation of the objectivity of justification will, in all likelihood, involve at least a nod in the direction of ontology, whether or not ontological considerations are brought in explicitly.

§ 5. Agreement and Descriptive Conventionalism.

We have thus far explored Stroud's claim that the true target of Wittgenstein's writings about necessity was Platonism. Wittgenstein's strategy of attack according to Stroud has been to show that the ontological basis of Platonist accounts of rule-following makes no

difference, that it cannot actually explain how it is that we come to grasp the objective meaning of mathematical and logical truths. This has been shown to imply two things. First; any rejection of the Platonist account will only be acceptable if can provide an alternative account of the objectivity of mathematical and logical truth. Second; any such alternative account which proposes to *explain* mathematical and logical necessity will need to appeal to our understanding or grasp of the rules of inference, and this appeal is likely to take said understanding as primitive—as an unanalyzable given. Finally, the way in which Wittgenstein's strategy of attack also mounts a challenge to traditional conventionalist accounts has been hinted at. I want to go on in the following chapters to demonstrate explicitly how the challenges to Platonism and conventionalism are made, but before so doing I think we need to take a quick glimpse ahead to see where we are headed. I have not said much so far about the alternative account of logical necessity Stroud thinks Wittgenstein *did* actually provide us with, apart from drawing attention to the fact that according to Stroud "it is not always easy to see how what he says differs from" the standard conventionalist account (Stroud 1966 p. 479). To this end it seems we can make headway if we attend in the first place to the style and strategy of Stroud's exposition of what he takes to be Wittgenstein's actual view on the matter.

It seems to me that Stroud's way around the difficulty of characterizing Wittgenstein's view as he sees it, is to focus upon the *descriptive* character of Wittgenstein's account. The importance of this move cannot be overstated. Many of the problems that are posed for conventionalism by Wittgenstein's writings stem from his examples. Exposition via example, which is one of his favorite devices, is very useful here. It allows *description* to take the main role and illustrate an insight with reference to a variety of cases. It is also perhaps a way to avoid actually *explaining* such concepts as general agreement and understanding or grasping a rule, which may prove to be a more successful strategy, and a less philosophically problematic one, in the long-run. One of the things these examples show is that any *explanation* of the source of logical necessity will not do. Indeed the explanation given us by radical conventionalism requires that we could find alternative

incommensurable conceptions of how to follow a rule equally acceptable. That we could actually accept and even adopt an alternative conception of the rule that would be incommensurable with everything else we believe and all our current practices. According to Stroud, the examples show that this possibility in particular ultimately ends up being unintelligible to us, we can imagine *other* beings having such unintelligible beliefs and practices, but that this unintelligibility is imaginable, does not make it any less unintelligible.

If the direction in which Stroud thinks we ought to take Wittgenstein's account is appropriate, and I think it might be, then *some sort* of conventionalism will be vindicated. The important thing to see is that this will be a *descriptive* as opposed to an *explanatory* sort of conventionalism, and it will not give us anywhere near as much as the explanatory form does. This idea has been suggested by Yemima Ben-Menahem, who argues in "Explanation and Description: Wittgenstein on Convention", that Wittgenstein actually subscribed to a "descriptive conventionalism" (Ben-Menahem 1998 pp. 109-19). Ben-Menahem tells us that:

The distinction between the descriptive and explanatory understandings of conventionalism reflects the general differences...between explanations and descriptions. Rule-following is a surface phenomenon. Rules are not hidden entities underlying phenomena: they do not antecede their applications as in a temporal or causal succession. They are neither hypotheses nor laws of nature. Hence, they do not explain their applications. Nor are rules responsible to antecedently-given meanings, concepts etc.... A rule and its applications are related as are a picture and its elements: they can be separately attended to, as one would attend to an element of a picture without seeing the picture or vice versa, but ultimately, any attempt to represent either one as fundamentally prior is bound to fail. (Ben-Menahem 1998 p. 116)

We will return in more detail to what Ben-Menahem has to say about descriptive conventionalism in the last chapter, but at this stage I want to draw particular attention to her point that "[a] rule and its applications are related as are a picture and its

elements...any attempt to represent either as fundamentally prior is bound to fail." I believe that a descriptive conventionalism would also be committed to a similar reappraisal of the relationship between understanding and agreement. Indeed, I think that such a reappraisal is what *stands beneath* the shift from explanatory to descriptive conventionalism. And that Stroud sought to draw attention to the fact that Wittgenstein actually gives us such a reappraisal, without actually being able to specify clearly the sort of conventionalism such a reappraisal yields. As part of such an account great stress needs to be laid on general agreement, not because following the rule is a matter of agreeing with others in what we do, but because that is how we *describe* what makes for following the rule. By stressing the importance of agreement in his examples Wittgenstein is not trying to suggest that we all share some understanding of the rule and that this explains why we agree about how to go on. He is drawing our attention to the role agreement plays in our descriptions of understanding or going on in the same way, or for example, in describing what it is to calculate. So we don't *explain* agreement by appeal to understanding (standard conventionalism), and we don't *explain* understanding by appeal to agreement (radical conventionalism), but instead we *describe* what understanding is for us by showing the role agreement plays in our rule-guided practices (descriptive conventionalism).

Stroud has a *lot* more to say about the role this notion of agreement plays. And in the last chapter I want to return to his discussion of the role played by agreement in Wittgenstein's account, and therefore to the sense in which Wittgenstein can be properly described as some kind of "conventionalist" (Stroud 1966 p. 490). For the moment though, two things should be kept in mind. First, "agreement" as used by Wittgenstein is not agreement in words, so to speak. That is, the notion of agreement is not one that can be renegotiated for example, or with which one could properly disagree in any way which would make sense to us. What this means is that the notion of agreement here is much more general than that of agreement in words, this is agreement not just in words, but in form of life (Stroud 1966 p. 494).

“So you are saying that human agreement decides what is true and what is false?”—It is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions but in form of life.
(PI §241)

Secondly, in connection with some of the things Ben-Menahem will have to say, we should keep in mind a remark of Dummett's about Wittgenstein's philosophy of mathematics and its links to intuitionism. For Dummett, as we have seen, the considerations about rules "belong to the theory of meaning in general, rather than having particular relevance to the philosophy of mathematics" (Dummett 1966 p. 428). I have argued against this that, at least in so far as the sorts of examples Dummett gives us go, his attribution of radical conventionalism to Wittgenstein is actually itself a move based upon considerations that more properly belong to the philosophy of meaning than to the philosophy of mathematics. That is, I have argued that Dummett is not in fact presenting arguments that deal with Wittgenstein's philosophy of mathematics, but arguments that deal with his philosophical view of meaning in general. There is a sense though, in which Dummett's critique of Wittgenstein does deal with issues going beyond the scope of his philosophical view on meaning. This is in the area of justification, the point which Dummett raises in this regard *does* properly belong to the philosophy of mathematics, and it could also be said to have significant relevance for epistemology. In the sentence immediately following the one above Dummett says that Wittgenstein "wishes, like the intuitionists, to insist that we cannot draw a line in advance round the possible forms of argument that may be used in mathematical proofs" (Dummett 1966 p. 429). I do not want to say too much more about this here, except to draw attention to the fact that in elucidating the notion of agreement that is central to Wittgenstein's philosophical view about meaning in general, it seems that we will have to deal to some extent with what he thought about justification, both mathematical and epistemic.⁷

⁷ If this paragraph is unclear, see the second paragraph of the last section, the subject matter is the same; the relationship between justification and meaning. The point is that knowledge will only be possible if we can show that this relationship is objective, which means we have to show how our grasp of a rule is related to our practice in a way which does not trade on traditional notions of understanding. If we cannot

§ 6. Digression I: The Rule-Following Considerations.

I want to finish this chapter by sketching out how I think Stroud argues that Wittgenstein's rule-following considerations, as explored through his examples of deviant rule-following, mount a challenge at one and the same time to Platonism and conventionalism. My purpose in so doing is twofold: in the first place we are going to need to have a framework within which the examples can be seen to be doing their job. And secondly, by undermining the particular conception of understanding that Platonist and standard conventionalist explanations of logical necessity share, the way is left open to also read the examples as promoting a descriptive conventionalism of the sort Ben-Menahem describes. The examples by themselves do not provide any sort of direct critique, rather *they direct our attention to certain things by dint of describing what is always before our eyes but never noticed*. It is important to be clear on this, and this is why we need to see, before we examine them in detail, how they fit into the larger project that Wittgenstein was pursuing with his later work. For clarity and brevity what follows is my own schematic representation of the challenges made by the rule-following considerations by way of adducing grounds for skepticism about rules and about understanding. Hopefully it will be clear how this sketch is supposed provide an understory for the analysis of the examples in the following two chapters.

There are many expositions of the rule-following considerations, with, Kripke (1982) and McGinn (1984) being amongst the best. I do not at this point want to go into too much detail with regard to these, but we will briefly consider those provided by the two authors mentioned later on. What I am interested to do is to work with what seems to me to be the main negative conclusion these considerations yield, and about which I think most of the foregoing authors would agree.

do this then either we accept the Platonist account, or we accept the skepticism we would be committed to in epistemology if we decided to adopt radical conventionalism as our theory of meaning. (See chapter 5.)

It seems to me that the main negative conclusion that the rule-following considerations give us, and which is explicated through Wittgenstein's examples, can be stated as follows.

(C1) There is no way to justify the claim that I am following any rules in particular.

We should remember that the term "justify" is only a device here. Although a reference to justification would usually imply that we are dealing with an epistemological issue, here this is not the case. The point is rather this: there is no way to show what the claim that I am following any rule(s) in particular comes to. This conclusion can be seen as being Janus-faced, with one face orientated to the rule-followed and one to the rule-follower. And it is each of these facets that challenges Platonism and conventionalism respectively. Let us deal with the face orientated to the rule-followed first. This conclusion could be stated as follows.

(C1.1) I cannot justify the claim that I am following any rules at all.

In brief this conclusion follows from the considerations about rules in the following way. Since at each application of a rule, there will be interpretations of the rule that agree in all applications of the rule up to this point but then diverge, I cannot look to the rule itself for guidance as to how to go on. If I claim to be following a rule then I should at each point go on in the right way, but since the rule cannot tell me which is the right way to go on, I will need a rule for interpreting the rule, but the problem is that the same difficulty arises with this new rule for interpreting the old one. In terms of justification: for there be a justification for the claim that I am following any rules at all, there needs to be some rule such that I do not need a rule for interpreting that rule for justification to get started, but since no rule can do this, it seems that all interpretations of the rule are equally justified, or as the case may be, unjustified.

This conclusion mounts a challenge to Platonism, because it undermines the Platonists' typical appeal to "the rule itself". That is, it undermines any appeal to an independently

existing rule that somehow intimates to me as to how I am follow it. For any rule that I am following and which does intimate to me how to follow it—short of its compelling me beyond my will, which does not seem to be a case of following rules anyway—I will need another rule that tells me how I am to respond to the intimations of the rule. For example if the order "collect all the red balls" is the rule I am to follow, and the rule intimates to me by bringing an image of a red ball before my mind, I am still going to need a further rule for interpreting that image, for example for interpreting it as representing a three-dimensional object rather than a two dimensional one, and so on.

The upshot of these considerations about this conclusion is that it directs us toward a skepticism about the idea that we are following any rules at all, and in this way it mounts a challenge to the Platonist metaphysic which makes explicit appeal to the existence of rules in and of themselves. This brings us to the second facet of the above conclusion that is orientated toward the rule-follower.

(C1.2) I cannot justify the claim that I am following some particular rule.

In brief this conclusion follows from the considerations about rules in the following way. Since it is always possible to interpret the rule in a way which diverges in application from the way in which I would usually go on, and since there are an infinite number of ways in which the interpretation might diverge in application, it is obvious that at each application of the rule there is no rule in particular that I can definitely be said to be following. Particularly when we remember that my current applications of the rule are only finite and each of the rules has a potentially infinite number of applications. With there being an infinite number of rules that diverge at any application from this one on but which are the same up until that point. In terms of justification: my claim to be following some rule in particular must involve the specification of which rule it is that I am following. Because at any point the rule I am following might be any one of an infinite number of rules that all diverge at this point, and because my past applications of the rule do not show which of these I might have been following. And since these considerations

apply to all of the potentially infinite applications of the rule I take myself to be following, I cannot justify the claim—made now—to be following any one of the rules in particular, all of which diverge from one another at each of the potentially infinite future applications that each may have.⁸

A good way to get a clear concept of this is to ask whether or not in some situation we are following more than one rule simultaneously. Might we not be following two rules which only diverge at some future point? And if this is so, how are we to justify the claim—either now or then—that we are actually following one of these rules and not the other? This is, in so far as I understand it, the reasoning behind Kripke's presentation of the rule-following considerations when he introduces his device of "quus" (Kripke 1982 pp. 7-21). As with the meeting with Kripke's bizarre skeptic, our belief that we were guided by one rule rather than another cannot provide what is needed to justify the claim that we are following any rule in particular.

We can now show how this conclusion mounts a challenge to the conventionalist. Stroud tells us that,

The crucial notion in this conventionalistic theory is that of understanding the meaning of a word or rule, and this is something to which Wittgenstein devotes a great deal of attention. Part of his interest is in the sense, if any, in which someone's having understood the instructions [for constructing a +2 series] somehow logically guarantees that he will write down "1002" right after "1000." (Stroud 1966 p. 480)

Now we have just seen how the third negative conclusion that we can draw from the rule-following considerations undermines the idea that we are able to justify the claim that we are following some particular rule. This is because at any point the rule that we are following could be one of any number of possible rules, all of which diverge at just this

⁸ An image that might be helpful here is that of an ever more complex branching of lines. If we think of such a situation where, say, our first line branches into three, and each of these branch-lines branch into three, and so on. Then we can see how, from our current standpoint, we might not be able to say which "line"—as in, which set of consecutive branch-lines—we might end up following.

point from one another. If we couple this consideration with the fact that the conventionalist seeks to reject the Platonic claim that there are absolutely right and wrong ways to go on, in say, inferring, then the claim that our "understanding the meaning of the word or rule...somehow logically guarantees that we will" go one way rather than another, is on shaky ground at best. This is because at any point the rule I *actually* understand might in fact be one that has me going on in a different way to the way we usually would. More to the point, since my "understanding" of the rule I am apparently following can only be gleaned from the finite number of applications of that rule that I have already been exposed to, and since any number of possible rules—possible ways of going on, continuing the series etc.—are consistent with *that* group of finite applications, there is no way to show how my "understanding" could logically guarantee how I will go on, because there is no way to show that I in fact understand any particular rule.

Again, we can approach this issue from another angle, and one more in keeping with the fact that we are dealing with a problem from the theory of meaning here, not from the philosophy of mathematics. It becomes a question of how is it possible for the understanding of a finite being to properly grasp a rule that has an infinite number of applications. Since a full grasp of the infinite number of correct applications of the rule is obviously beyond the capacity of a finite being, this becomes a puzzle about how understanding the rule guarantees that the rule-follower will go on in the right way. As we mentioned earlier it seems that the rule-follower might even be following two different rules simultaneously, perhaps even two rules they would not be able to tell apart were it not shown to them that they are separate. Wittgenstein puts this eloquently himself in a passage from *Philosophical Grammar* when he describes the following situation (again, exposition by example).

If I *succeed* in reproducing a paradigm in accordance with a prescribed rule, is it possible to use a different general rule to describe the process of copying, the way it took place? Or can I reject such a description with the words "No, I was guided by *this* rule, and not by the other, though admittedly in this case

the other would have given the same result."? (Wittgenstein 1974 §57 pp. 98)

The point here is that until some difference appears in my actual practice, if it ever does, then there is no way to say for sure which of several rules is the one I am following. Of course we may, as Wittgenstein says here, want to dispute the description of us as following some rule other than the particular one we take ourselves to be following. But we cannot appeal to that description to show that that rule is in fact the one that we were actually following. For such a claim contains the implicit assumption that there is just one way to correctly interpret the rule, but the problem with this assumption is that it relies upon the very notion of what it is to correctly follow a rule that we have sought to explain. In other words, the claim that we actively interpret the rule as we apply it commits us to giving some account of what constitutes the correctness of our application, what shows that we are still following the same rule, and indeed, what shows that we are only following the one rule. If the claim that we actively interpret the rule in applying it is to stand up it must show how we tell we are following some particular rule; and it will be no use to say, "but I am doing the same thing over and over", for this relies upon the notion of sameness. As Wittgenstein points out, it is not at all clear what counts as sameness, unless we know what rule it is we are following in the first place.⁹ And of course to identify sameness and difference is itself to follow a rule which if we interpret it by applying it stands in need of just the same sort of supplementary account of what it is to apply it correctly, or indeed to apply it at all. And this brings us back around to the skepticism, engendered by (C 1.1), that we thus have reason to doubt that we are in fact following any rules at all.

⁹ Stroud himself takes a reference for this from *Remarks on the Foundations of Mathematics* I §113. I prefer *Philosophical Investigations* §§60-3 and §§207-21, both of which relate the notion of sameness to following rules.

Chapter 3

Mistakes and Misunderstanding.

§ 1. Introduction.

So far we have set the scene for a more thorough discussion of Stroud's claim that Wittgenstein was not a radical or "full-blooded" conventionalist as Dummett maintains. When laid out in schematic form we can see that the rule-following considerations; which are constituted by the very examples Dummett finds "thin and unconvincing"; pose challenges to: (a) the Platonism that Stroud identifies as Wittgenstein's main target, and (b) standard conventionalism as favored by the logical positivists. The most important thing about the case Stroud makes for Wittgenstein, is the way he uses Wittgenstein's own examples to expose the critiques of both Platonism and conventionalism that flow from the rule-following considerations. These examples are predominately of two types and usher in the most important arguments for skepticism about Platonist and conventionalist explanations of the source of logical necessity. The first type of example, as examined below, involves only an individual. The second type involves a whole society. I believe that Stroud's most important contribution to this debate has been his explanation of the role examples of the second type play in Wittgenstein's account. Although we will discuss them at length in the next chapter I feel that at least this much must be said about these examples at present.

The examples of the second type do not just provide us with grounds for skepticism about rules and the understanding or grasp we have of them. They also provide us with material of a more positive nature. They provide us with a clue as to the sort of conventionalism Wittgenstein *did* favor. This makes the examples of the second type distinct from those of the first type. The first type of examples are, to my mind, entirely negative in their impact, their sole function is to undermine the claim that there is any *absolutely correct* way to go on when following a rule. And in this way they successfully

undermine both Platonism and standard conventionalism. We will return to this issue in more depth in the following and the final chapters of this thesis

So in this chapter we move on to consider the first type of example. Although these examples mount different challenges to Platonism and conventionalism, their deployment by Wittgenstein can be seen as a part of a single strategy. It seems to me that Stroud is concerned not just to dispute Dummett's radical conventionalist claim, but also to draw our attention to this strategy of argument that Wittgenstein is using. I will have more to say about this later when both sorts of examples have been considered, but we can note for the moment that the general *function* of the first type of example in Wittgenstein's assaults on Platonism and conventionalism is the same. These examples are used to make essentially the same logical point: there is no *absolutely correct* way to go on in following a rule. That is, in terms of logical compulsion, such compulsion cannot come from any source but our own adherence to the principles of logic. As Ben-Menahem says, the point is to show that the relation between a rule and its applications is not "external" to language, and cannot be *explained* by appeal to "extra-linguistic" facts (Ben-Menahem 1998 pp. 115-6).

Before we go any further something must be established. In his "elementary exposition" of the rule-following considerations, Saul Kripke makes a short digression toward the end of the second chapter of his book to briefly discuss the challenge his Wittgensteinian paradox makes to Platonist accounts (Kripke 1982 pp. 53-4). As Kripke rightly points out, the resources are there within such accounts to explicate not only how the specific instances of a rule are related to the rule itself, but also to explain how it is that coming to grasp the rule ensures that I will go on to make the correct application of it in each future case.¹⁰ The weak link, according to Kripke, is not that between rule and instance, but between the rule as an objective mathematical entity and my subjective grasp

¹⁰ Instances and applications of a rule are not the same. Rules only have instances in so far as they are viewed as objective. That is, the full table for, say, the +2 series would list all the instances of that rule. An application of a rule is what a rule-follower does with it, and rules have applications whether viewed objectively or not, but you cannot list all the applications of a rule ahead of time.

of the rule (Kripke 1982 p. 54). Once I have grasped the rule, there ought to be no problem, according to the Platonist, with my going on to apply it in the correct way a potentially infinite number of times. This echoes a point made earlier by Dummett himself, that "if we believe in the objectivity of mathematics, then there is no objection to our thinking in terms of mathematical objects, nor to the picture of them as already there waiting to be discovered that goes with it" (Dummett 1966 p. 423). And this just because on this view the relationship between a rule and its instances is "external" to the practice of actually applying the rule. This means that if I have grasped the rule then I will have grasped all of its instances and so how it is to be applied at each step. The upshot of this is that the examples of the untutorable pupil that play such a central role in the rule-following considerations, only really mount a challenge to Platonism in so far as they mount a challenge to the individuals *understanding* or *grasp* of the rule. Even in the case of, as it were, making an instance into an application, it is my *grasp* of the rule that determines how that instance is realized as an application. So while the rule-following considerations might appear to challenge Platonism on *two* fronts; the first involving the interpretation of the rule as "read off" reality, and the second involving the interpretation of the rule as to what counts as a correct application in any situation. In fact these challenges are one and the same, they are challenges to my subjective grasp of an objective rule, to my *understanding* of it.

This is the reason I think Dummett is wrong when he claims that what is being dealt with here is Wittgenstein's philosophy of mathematics and not his philosophy of meaning. In so far as Wittgenstein seeks to challenge Platonism at all, it is on the basis of the Platonists claim that we subjectively grasp the objective meaning of the rule, and thereby go on to apply it correctly. That is, the rule-following considerations, including Kripke's Wittgensteinian paradox, only challenge Platonism by challenging what the Platonist says about our understanding of the rule. And perhaps apart from the spurious claim that "talk of abstract entities" makes no sense and unnecessarily bloats our

ontology, there may not be any real basis to challenge the Platonist account otherwise (Dummett 1966 p. 423).

§ 2. An Untutorable Pupil.

Type one examples involve individuals, or as I prefer to call them ‘deviants’. These ‘deviant’ examples, are examples that involve an individual who is part of our society, but who is either incapable of following rules, or whose understanding of them is radically different to our own. Typically the deviant examples involve a pupil who seems resistant to, or unable to respond to their training. The most famous of these is, of course, that involving the pupil who goes on to construct a number series after being given some basic training, and some preliminary testing. There are many samples of this type of example in Wittgenstein’s work, but I think that we can draw out what Stroud thinks they show with greater clarity if we concentrate on just one and give it full exposition. The following two passages, §143 and §185 of *Philosophical Investigations*, both treat on the same basic example, that of an untutorable pupil, with the latter being an explicit expansion of the former. We should also remember that this example is framed from the point of view of the teacher, not the pupil. This will help us to avoid getting side-tracked into a discussion, too large to engage in here, about understanding not being a mental state.

143. Let us now examine the following kind of language-game: when A gives an order B has to write down series of signs according to a certain formation rule.

The first of these series is meant to be that of the natural numbers in decimal notation.—How does he get to understand this notation?—First of all series of numbers will be written down for him and he will be required to copy them. (Do not balk at the expression "series of numbers"; it is not being used wrongly here.) And here already there is a normal and an abnormal learner's reaction.—At first perhaps we guide his hand in writing out the series 0 to 9; but then the *possibility of getting him to understand* will depend on his going on to write it down independently.—And here we can imagine, e.g., that he does copy the figures independently, but not in the right order: he writes sometimes one sometimes another at random. And then communication stops

at *that* point.—Or again, he makes '*mistakes*' in the order.—The difference between this and the first case will of course be one of frequency.—Or he makes a *systematic* mistake; for example, he copies every other number, or he copies the series 0, 1, 2, 3, 4, 5,...like this" 1, 0, 3, 2, 5, 4,... Here we shall almost be tempted to say that he has understood *wrong*.

Notice, however, that there is no sharp distinction between a random mistake and a systematic one. That is, between what you are inclined to call "random" and what "systematic".

Perhaps it is possible to wean him from the systematic mistake (as from a bad habit). Or perhaps one accepts his way of copying and tries to teach him ours as an offshoot, a variant of his.—And here too our pupil's capacity to learn may come to an end.

185. Let us return to our example (143). Now—judging by the usual criteria—the pupil has mastered the series of natural numbers. Next we teach him to write down other series of cardinal numbers and get him to the point of writing down series of the form

$$0, n, 2n, 3n, \text{ etc.}$$

at an order of the form "+n"; so at the order "+1" he writes down the series of natural numbers.—Let us suppose we have done exercises and given him tests up to 1000.

Now we get the pupil to continue a series (say +2) beyond 1000—and he writes 1000, 1004, 1008, 1012.

We say "Look what you've done!"—He doesn't understand. We say: "You were meant to add *two*: look how you began the series!"—He answers "Yes, isn't it right? I thought that was how I was *meant* to do it."——Or suppose he pointed to the series and said "But I went on in the same way."—It would now be no use to say: "But can't you see...?"—and repeat the old examples and explanations. In such a case we might say, perhaps: It comes natural to this person to understand our order with our explanations as we should understand the order: "Add 2 up to 1000, 4 up to 2000, 6 up to 3000, and so on."

Such a case would present similarities with one in which a person naturally reacted to the gesture of pointing with the hand by looking in the direction of the line from finger-tip to wrist, not from wrist to finger-tip.

In this example, the pupil is taught to write down a series first by being guided and imitating the teacher, and then by going on to copy the series off independently, so far they are merely copying, their rule is a rule for translating, for example, typeface into script. The possibility of mistranslation or mistake is also raised, and the idea that it would be possible for the pupil to understand wrongly is also alluded to. Later we are to imagine they are taught how to construct a series by adding the same number each time, and are then given various tests. We are to understand that the pupil performs satisfactorily in all tests and that on the basis of their performance so far, they are entitled, at least initially, to claim to have mastered the technique we have been teaching them. We then set the pupil a test which goes beyond their training, and to our surprise the result is that, by our lights, they fail the test. Yet, when we take them back over the training we find that they seem to understand it in the way we would expect someone who has mastered the technique to. Not only that, but when we try to point out that they have at some point in their construction of the series ceased to go on in the usual way and have started to go on differently, they do not understand, and indeed, they maintain that they are now going on as they always have.

Stroud says that what Wittgenstein is trying to do with these deviant examples is to use them to raise certain questions about what it is to follow a rule correctly; in particular, “what is it to understand a rule correctly? What determines which move is the correct one at a given point?” (Stroud 1966 p. 481). Again, if the response to these questions is something like, to understand correctly is to understand the way the instructions were meant, then the question can be rephrased as a question about what shows which way the instructions or orders were meant; “[b]ut again, Wittgenstein asks, what shows which way the order was meant?” (Stroud 1966 p. 481).

Finally, we should also remember that the idea that the teacher very briefly thought through the whole series when giving the order is also to be rejected. After all, the instructions themselves don't contain the rest of the series. "Start at zero and go on to

construct a series by adding two." Does not mean. "Write down '0,' then '2,' then '4,' then..." and so on infinitely. Though the two are equally good descriptions of what one should do, they do not have the same meaning, for if they did thinking one was following a rule would be the same thing as following it, but this is not so.

§ 3. Mistakes and Understanding Wrongly.

According to Stroud, Wittgenstein raises these questions to bring to our attention the connections between our use of words like "understand", and "know", and phrases like "Now I can go on". And in contrast to this our use of words like "misunderstand" and "mistake" in describing such situations. Exactly what it is that we are to see here is probably best brought out by way of a rhetorical question: are all cases of going on differently in following a rule simply cases of misunderstanding (whether that be *misreading* of the rule, as a Platonist would have it, or *misinterpreting* the rule as the conventionalist would prefer)? The way we answer this question is supposed to show us something about what we call "understanding" by reminding us of what we actually mean when we say that someone *misunderstands*. But before we go any further we are going to need to get clear about the difference between misunderstanding and understanding wrongly, and to do this we need to get clear about exactly what a mistake is.

Section 143 begins with the pupil being taught how to copy out a series of numbers.¹¹ The pupil's hand is first guided by the teacher, and then they are to imitate what they see the teacher do. We are then told that "*the possibility of getting him to understand*" rests upon the pupil being able to make the move from imitation to independently copying the series down (PI §143). We are then given various descriptions of how he gets it wrong; he writes the numbers down at random, or he makes mistakes in the order, or he makes a

¹¹ It seems to me that Wittgenstein's comment that the expression "series of numbers" is not being used wrongly in this passage relates to the idea that what the pupil copies can only be a series of numbers if he recognizes it as such. As I mentioned earlier, I do not want to get into what would have to be a very long discussion about Wittgenstein's view that understanding is not a mental state as such. But it seems to me that what he means by this remark is that its being a number series has nothing to do with whether or not the pupil himself recognizes it as such. And in the last chapter I hope to show why this is so.

systematic mistake which involves getting the order wrong in what we might call a “patterned” way. And this brings us to the idea of understanding wrongly. In connection with his example of a systematic mistake Wittgenstein says: "Here we shall almost be tempted to say that he has understood *wrong*" (PI §143). He then says:

Notice, however, that there is no sharp distinction between a random mistake and a systematic one. That is, between what you are inclined to call "random" and what "systematic". (PI §143)

If there is no sharp distinction here, then it also seems that there can be no sharp distinction between understanding wrongly and being sloppy or careless in copying the series down. We have to remember that we are dealing here *only* with a situation in which a pupil is copying out the series of numbers, *not* one in which they are constructing the series. *So there can only be a mistake if there is definitely something to get wrong.* In this case, the thing to get wrong is the reproduction or translation of the typeface into script. The fact that the pupil may get this wrong in a patterned way, is what allows for the idea that they might have “understood *wrong*” and Wittgenstein says of this situation that:

Perhaps it is possible to wean him from the systematic mistake (as from a bad habit). Or perhaps one accepts his way of copying and tries to teach him ours as an offshoot, a variant of his.—And here too our pupil's capacity to learn may come to an end. (PI §143)

It seems to me that there is something very important about the idea of understanding wrongly as opposed to misunderstanding. To say that someone has "understood *wrong*" seems to imply that there is the possibility of grafting an appropriate understanding onto an inappropriate one, that we could "wean him from a systematic mistake (as from a bad habit)." It seems to follow from this that the sense in which the pupil understands when he goes on to copy down the series independently, is not the sense in which he understands when he understands how to construct the series independently. *This strikes me as important, because I think herein two issues are clarified.*

First of all, we should cast our minds back to the example that Dummett himself gives us of the problem which Wittgenstein's view, as he reads him, poses for our understanding of mathematical necessity. Dummett's example, we will remember, involves an exercise in counting children. Our counter initially has only one criterion for determining the number: counting. He is then introduced to the new criterion of addition, and concurrently to the idea that additively discordant results are a symptom of his having miscounted. In response to Wittgenstein's perceived insistence that the two criteria are distinct rules of language, Dummett responds that were this so they would then be able to conflict, saying "[b]ut this is absurd: one cannot make some mistake without there having been some particular mistake which one has made" (Dummett 1966 p. 432).

Aside from the fact that I don't think Wittgenstein's views commit him to this, it seems to me that Dummett in fact has misunderstanding and mistake mixed up here. With reference to the above discussion it should be apparent that making a mistake involves there already being a sanctioned way to go on. While Dummett's example is more complex than this, the point is that we don't so much have a mistake here as a misunderstanding. The counter has not failed to employ a criterion correctly, or consistently misemployed it; he has not even properly grasped it in the first place, or at least so it seems from the information we are given. In a sense Dummett is correct, and it is just because "one cannot make some mistake without there having been some particular mistake which one has made" that mistakes can in the first place be identified as, random, irregular, or systematic. But misunderstanding cannot be any of these.¹²

Secondly, and only briefly. I said that there seems to be more than one sense in which one can understand; there can be understanding as is manifested in the ability to copy down the series independently, and there can be understanding as is manifested in the ability to construct the series. These do not seem the same. The first type of

¹² I think it could also be argued that in the case of machine-as-symbol vs. machine-as-concrete this argument might also be relevant. The machine-as-symbol cannot make mistakes because it is conceived as not so doing. The machine-as-concrete can malfunction, and I take this to be a correlate of human mistake. After all, malfunctions can have random, irregular, and systematic effects.

understanding seems to be more skill-like than the second. Does this mean the second is involves something more than a skill? I believe the answer to this question is in fact “No”, but I think that this will only become obvious when we reach the last chapter. I can say in anticipation thought, that it is has something to do with the fact that saying someone “understands” is a *description*, not an *explanation*, of their ability to go on.

§ 4. Misunderstanding.

We can now return to the idea of misunderstanding. When we say that someone misunderstands, we generally intend to convey more than that they have just made some mistakes. To make the judgment that misunderstanding has indeed set in we need some kind of criteria, the criteria available seem to form a spectrum. At one end we have random mistakes, further along systematic mistakes, and at the opposite end complete communication breakdown. It seems difficult to believe that irrevocable communication breakdown could actually qualify as misunderstanding. On the other hand, it seems too hasty to say that an occasional random mistake shows that there is genuine misunderstanding present as opposed to, say, sloppiness on the part of the pupil in carrying out the order. And as we have already seen, a systematic mistake seems to more of an indication that the pupil has “understood *wrong*” than that he as not understood at all. This leaves us the distance between systematic mistake and complete communication breakdown in which to locate misunderstanding. And yet, I think that we will find that it cannot be located there either. While the main symptom for us of a pupil's genuine misunderstanding might be that we cannot see what he means by saying he is going on in the same way, this might not be the same point at which misunderstanding has actually set it. And all of this tells us something about the grammar of the words “understand” and “misunderstand”; but more later.

Despite this we are told that “*the possibility of getting him to understand*” rests upon the pupil being able to make the move from imitation to independently going on in the same way (PI §143). So, understanding requires that the pupil is able to at least begin to

execute the order without assistance, that he is at least able to start going on as we would. We should note then that *misunderstanding* can only genuinely occur when the pupil has gone beyond imitating the teacher, and so both *misunderstanding* and *understanding* already require more than just doing as others do. They require doing as others *would* do.

Section 185 picks up at the point where the pupil is considered to have mastered the series of natural numbers. There is a definite jump here. We have moved from the pupil being able to copy down the series correctly without assistance to their being able to construct the series of natural numbers on their own. The intervening passages between §§143-85 deal mainly with aspects of this transition. But even so I think that the fact that 185 refers directly back to 143 alludes to something important. To my mind Wittgenstein is alluding to the fact that, from the point of view of the teacher, not much else is actually involved in mastering the technique of constructing the series of natural numbers than is involved in mastering the technique of writing down the series of natural numbers. While we might have to take the pupil through the process of writing down the series various times until by our lights they have got it right, in the end the only criterion we have for establishing whether or not we have properly imparted this knowledge will be that the pupil does indeed go on to write down the series in the usual way.

Next we are told that the pupil has been taught, seemingly on the basis of their mastery of the series of natural numbers, to write down series of cardinal numbers at the order “ $+n$ ”, of which the series of natural numbers is a special case; and at this point Wittgenstein also gives us the general formation rule for the construction of such series, “0, n , $2n$, $3n$, etc.” (PI §185) We are also told that the pupil has been given “exercises...and tests up to 1000”, but that on the order to continue the series “ $+2$ ” beyond “1000” the pupil goes on “1000, 1004, 1008, 1012...” (PI §185) Then we get the following sample exchanges.

We say “Look what you've done!”—He doesn't understand. We say: “You were meant to add *two*: look how you began the series!”—He answers “Yes,

isn't it right? I thought that was how I was *meant* to do it."——Or suppose he pointed to the series and said "But I went on in the same way."——It would now be no use to say: "But can't you see...?"——and repeat the old examples and explanations. (PI §185)

There are two possible exchanges, but the general point of both is the same, and perhaps Wittgenstein wants to be clear that this is not a precocious pupil intent on misbehaving. What seems of most import is the last line of the paragraph: "It would now be no use to say: "But can't your see...?"——and repeat the old examples and explanations." Again we have an untutorable pupil, but *this time* it is because they have misunderstood, or don't understand, *not* because they are mistaken. And Wittgenstein has made the important observation that it is not clear at which point we should claim that the pupil has actually begun to misunderstand.

The following diagram from the *Brown Book* will perhaps make this clearer.

.....

In our figure a row of dots with large intervals succeeds a row of dots with small intervals. Which is the last dot in the first sequence and which the first dot in the second? (BB p. 121)

Though this diagram is presented as part of what Wittgenstein has to say about reading, we can make use of it, and for our purposes the analogy is the same analogy.¹³ When did

¹³ This point merits more in-depth discussion which it would take me too far afield to delve into here. Hopefully a few sparse remarks will suffice to indicate what I think is important. The whole of the discussion of reading that occurs in *Philosophical Investigations* (§§156-71), and the preparatory form that it has in *The Blue and Brown Books* (1964 §§66-73 or pp. 119-25) is relevant to our concerns here. The main points that should be made are that of all of the examples of mastering a skill that Wittgenstein uses, his treatment of reading is probably the most sustained. Again, the discussion at these points is not a discussion about understanding what is read, but just about the activity of, as it were, translating the marks that the eyes pass over into another form, either words or copy-script. This is important, because Wittgenstein's attack on Platonism treats the issue of whether we *could* ever come to read things off from reality, distinctly to the issue of whether or not our understanding of, say, mathematics, is a matter of discovery or a matter of invention. That is, Wittgenstein gives us reason for being skeptical about two things here: about whether or not it is possible to say that there will be just one correct translation scheme for, say, some series of marks on a piece of paper; and about whether someone has actually understood anything, indeed whether they had any properly *mental* operations at all going on, when they were engaged in exercising the skill involved in making such a translation. I believe that Quine's notion of the indeterminacy of translation is basically another manifestation of the first sort of skepticism gestured to here, skepticism about the idea that there could ever be such a thing as *the* translation of an object language into a target language. And as I have said, it seems to me that this sort of skepticism makes an implicit challenge to the distinctive feature of the Platonist account: the claim that there are

the pupil first misunderstand the series? In the transition from “1000” to “1004”? But then what are we to say of a slip of the tongue, or simple mistake? In the transition from “1004” to “1008”? But this itself does look like a continuation of some series, albeit a different one from the one continued in the transition from “998” to “1000”, so perhaps the pupil has only understood wrongly?

In fact the change is only such as a result of what surrounds it, what occurs on either side of the “1000, 1004” transition is all that tells us that the series being constructed has changed, but this still does not pin-point the change. In fact, if the pupil does “understand our order...as *we* should... “Add 2 up to 1000, 4 up to 2000...”” then it seems that they might have been going on in a different way from zero, and that we could just not tell (PI §185). Again, there seem to be a couple of ways we could put this. If the pupil has understood wrongly in the way just described, how are we to describe the misunderstanding itself, is it a misunderstanding from “0”? Or just from some point between “0” and “1000”, and if so where? The pupil cannot think of the whole series ahead of time, so at what point did it seem natural to them to move from “1000” to “1004”, at just that point, or two moves before that point, and why not three moves before that point, or four? We are running up against the walls of our cage here, for the pupil the “998”, “1000” transition is apparently exactly like the “1000”, “1004” transition: there has been no change in the size of the steps taken. We don’t identify when the change in the pupil’s understanding occurs—where misunderstanding sets in—we identify where

independently existing rules that we “read off” from reality. The second sort of skepticism could be seen as similar to the sort of skepticism about the existence of minds—either others or my own—that is the most distinctive feature of behaviorism. This other skepticism, while not of the behaviorist species, shares enough with it to undermine the mystical notion of understanding that traditionally forms the basis of conventionalist accounts of rule-following. At the same time, I think that it is clear that (contra-Ben-Menahem 1998 p. 125) there is no correlate of Quine’s holism about language here, and in this respect Wittgenstein’s account is distinct. The idea is that skepticism about “understanding” *a la* conventionalism, while similar to, is not a species of, skepticism about minds in general *ala* behaviorism. Skepticism about minds in general means that any account of meaning will have to take language as a whole into account; will have to deal with the total complex of activities called “language”, the totality of dispositions to verbal behavior. This is because all that one has as clues to the meaning of any fragment of the whole is the rest of the whole from which it comes. Each fragment, sentence or whatever, is like a piece of a jigsaw that only becomes that piece in the context of the overall puzzle (See Kripke 1982 pp. 14-5 for another way to argue the same point, and pp. 46-51, including footnotes, for other evidence to back it up). Skepticism about understanding only would not seem to have such implications.

their performance diverges from our own, and these two are quite different.¹⁴ We can say where by our lights they started to go on differently, but it is obvious that the point *we* pick for the change occurring is not the only point it might have occurred at, *and is only the point at which the difference strikes us*, at which we would take ourselves to be going on differently if we were constructing the series. It should therefore be clear what Wittgenstein means when, as Stroud says, he asks “who says what ‘change’ and ‘remaining the same’ mean here” (RFM, §113). And Stroud goes on.

One is inclined to reply, I think, that nobody *says* what is the same and what is different; it is just a fact that the pupil is wrong in supposing that going on “1004, 1008...” is doing the same as he was in writing down “2, 4, 6....” But is there some discoverable fact of which we are aware, and which he is missing? What sort of fact is it, and how could he be brought to acknowledge it? (Stroud 1966 p. 483)

The same notion is an integral part of Saul Kripke’s reconstruction of what he takes to be the challenge made by Wittgenstein’s skeptical paradox. When Kripke imagines the “bizarre skeptic” confronting him and challenging the answer of “125” that he gives to the equation “ $68 + 57 = ?$ ”, he points out that the skeptic is implicitly raising this very idea. The skeptic picks the current answer to be the first deviant answer, the first answer which involves us going on in a way different to the way we have been going on; we have been quadding up until this point, and now we have started to add, and hence our practice has changed, but we just don’t recognize this. The whole reason that the skeptic can actually make such a claim is that we can apparently never be sure that our practice has *not changed*, and as such we must grant the skeptic his point. The problem is actually a bit more complex than this, because two things change. The skeptic also proposes that we meant “quus” by “plus” in the *past*, and that to be consistent with our previous intentions now we need to answer “5” and not “125”. Though he does not dispute that we *now* mean what we usually do by “plus”, and the correctness of “125” as the answer to “68

¹⁴For another discussion of this same point made with specific reference to the role deriving plays in the process of reading see PI §§162-4.

plus 57 = ?" (Kripke 1982 pp. 14-5). So in fact two things have changed, our use of "plus" to mean what we usually do, *and* our practice, because we have ceased quadding any longer and have begun adding according to the skeptic. I think it still stands that Stroud's point is implicitly stated in Kripke's exposition of the paradox, though it behooves us to remember that Kripke's Wittgensteinian paradox is more complex, encompassing what flows from Stroud's treatment of *both* individual deviant and alien society examples of following rules in unusual ways.

Let us restate Wittgenstein's important observation for the sake of clarity. He says that "[i]t would now be no use...[to] repeat the old examples and explanations" (PI §185). Why would it be of no use? Well the answer is given to us in the next lines where we are told that perhaps it "comes natural to this person to understand our order with our explanations as we understand...", with the alternative rule being specified (PI §185). The important observation buried herein, but made explicit at many other points in his work, is that from the pupil's performance we have *no real indication whatsoever* as to when their practice might have changed.¹⁵ As the alternative rule specified by Wittgenstein exemplifies, the deviant pupils practice may have be different from the very start; just because it only becomes apparent to *us* at the "1000, 1004" transition, this does not mean that it originated in this transition, and indeed, there seems no way to tell where it might have originated.

What are we therefore to say of someone who goes on differently, and persists in so doing, after initially going on just as we do? It is as if someone lured us into a false sense of security, and then promptly pulled the rug out. What are we to say of someone who seems to be very much like us, who behaves very much like we do, and who after a certain point changes and persists in taking a different direction? In particular, do we have any right to tell them they are wrong? And in what is this right rooted? What grounds do

¹⁵ For examples of this see PI §§151-2 and §§162-4.

we have for thinking that *we* have it right and they do not? Stroud probably broaches this point with most clarity in the following paragraph.

If someone who had learned to continue various series just as we do began to differ from us when he went beyond any point he had reached in his training, would it follow that he simply had not understood the instructions? If he continued to do this, must we say that he is unintelligent, perhaps idiotic? Wittgenstein tries to suggest negative answers to these questions.... He tries to show that not all cases of deviating from what we expect or from what we all do in continuing the series can be put down to simple misunderstanding, stupidity, or deliberate perversity on the part of the pupil. It is almost certain in any particular case we come across that some discoverable mistake has occurred, and that the pupil will come to recognize this. But *must* he do so? Is there no possibility other than those mentioned above? The example[s are] intended to suggest that there is. But the important, and difficult, problem is to say exactly what this alleged possibility comes to. Although Frege said it would be a new kind of insanity, "he never said what this 'insanity' would really be like" (*RFM*, I, 151). To see what it would be like is to understand on what our being compelled in inferring, calculating, counting, and so forth, rests. (Stroud 1966 p. 481)

So according to Stroud's Wittgenstein we are to conclude that not all cases in which someone goes on in a different way to the usual are cases in which we could properly describe him as having misunderstood what he was told. And that not all cases in which he continues in this way, even after we have tried to show him how he is going wrong by our lights, are cases in which we must say that he is unintelligent or perhaps idiotic. This conclusion has some important implications for both Platonism and conventionalism, because as much as it is a conclusion about *mis*understanding it is also a conclusion about understanding. If we cannot establish when someone who goes on differently from the usual actually began to misunderstand, we cannot therefore establish exactly what it is that their misunderstanding consists in. As such we cannot therefore describe it as "understanding wrongly", because we cannot show how their understanding is actually wrong. The implication here is that if we cannot show how they have got it wrong, we

cannot justify the claim that they have indeed gotten it wrong. This implies something startling: if it is true that we cannot justify the claim that they have gotten it wrong, then it also seems unclear what getting it right amounts to. If getting it right, or understanding correctly, is to amount to more than just arbitrary stipulation, then we would seem to need some justification for demarcating some ways of going on as right and others as wrong. But if it is not possible for us to clearly establish *at what point* someone has gotten it wrong, then not only will we be unable to give an explicit justification for making such a demarcation; we will not even know where the line is to be drawn.¹⁶ Did our deviant pupil correctly understand the rule at "204, 207, 210" but only cease to understand it at "210, 212, 214"? Can we say that the only reason that they can be said to have gotten it wrong is that we are not inclined to agree with them, and is it only such agreement between ourselves that explains why we feel compelled to go on from "if p then q" and "p" to conclude "q"? The answers to these questions await the next chapter and beyond. We shall for the moment turn our attention to how these implications impact upon Platonist and conventionalist accounts of rule-following, by looking at what sorts of answers they would propose to above questions, and in what respects those answers fall short.

§ 5. Implications for Understanding: Platonism.

In the conclusion of the last section I said that since we cannot establish when the deviant pupil actually first began to misunderstand, we cannot say that they have understood wrongly, and therefore possibly "wean him from the systematic mistake (as from a bad habit)." (PI §143) In section 185 of *Philosophical Investigations* Wittgenstein says that we might say of the deviant pupil that he understands our order and explanations

¹⁶ Inexplicit justification or drawing a blurry demarcation line, though possible, will not be enough for either the Platonist or the conventionalist. And as we will see, that we can, and in fact do, do this only speaks in Wittgenstein's favor anyway. Ben-Menahem speaks of a similar, and perhaps even the same, contrast between "philosophical" and "innocent" uses of the terms "justification" and "explanation" in such contexts; and I think she seeks to foreground the same basic point (Ben-Menahem 1998 pp. 115-7). And it might be said that at least one of my aims in this thesis—see the last chapter for this—is to show that Wittgenstein can be read as holding that such a contrast also exists, though in a slightly different way, between different senses of the term "agreement" within both philosophical and ordinary language.

as we would those for a rule like; “[a]dd 2 up to 1000, 4 up to 2000, 6 up to 3000, and so on.” Although this might look like we could therefore say how it is that they have gotten it wrong, this is in fact not so. Even if we can see how the way the pupil goes on reflects some other rule that we might be able to formulate, or that on some occasions we might even follow, this by no means gives us any reason to think that this fact makes us any more able to teach them, or to secure a correct understanding on their part. No matter how we explain the difference to them between the rule we want them to follow and the rule they seem to be following, it may still turn out that we are unable to teach them. Why? Because to thus explain to them the difference between the rule we want them to follow and the one by our lights they are following, we have to invoke yet another rule for interpreting a rule, but then the same problem could arise with this new rule as arose with the one for constructing the series in the first place. Hence the similarity to someone, “who naturally reacted to the gesture of pointing with the hand by looking in the direction from finger-tip to wrist”. Stroud has a truly ingenious way of putting this;

If that person naturally reacted to the gesture of pointing by looking in the direction of the line from fingertip to wrist, it would not be enough to say to him, “If I point this way (pointing with my right hand) I mean that you should go *this* way (pointing with my left hand in the same direction).” Isn’t every explanation of how someone should follow an arrow in the position of another arrow (*BB*, p. 97)? (Stroud 1966 p. 483)

We can thus conclude that no matter how full our explanation, nor how precisely we formulate the rule for forming the series, nor how much training we give our pupil in the first place (i.e. no matter how large its scope) we can never be sure that we will be able to get the deviant pupil to master the technique we are trying to teach them, to construct the series as we do. Indeed, even in the case of non-deviant pupils, it would seem that it is only by chance that they master the series at all, since it is always possible that at some point they will, according to rest of us, begin to go on in a different way to the way they have so far gone on.

This is the way in which the rule-following considerations as exposed through Wittgenstein's examples mount a challenge to the Platonism of Frege and Russell. Even in the case of non-deviant rule-followers, it is not possible to show that the ontological claim, that there are independently existing rules that we read off from reality, gives us any reason to think that there is any absolutely *right* way to go on. Even if there were such rules in reality that we could read off, we could not appeal to them in teaching a pupil how to go on when enumerating the elements of a series, because to do so would involve us in a regress of rules. And, since it is not possible to say at what point a pupil either begins or ceases to grasp the rule we want them to, we would not be able to tell when they have begun to read off the rule we are thinking of anyway. It remains for us to show how these examples also show how the rule-following considerations mount a challenge to the conventionalist account of what it is to understand a rule, and we begin doing that by returning to the sense in which we need not say that the deviant who goes on differently has actually misunderstood the rule we sought to impart to them.

§ 6. Implications for Understanding: Conventionalism.

Now to say that someone who goes on differently has misunderstood what they were told, we must be able to point to something that shows that they should have gone on in the way that we would, and not as they have. If, as has been shown contra Platonism, we cannot point to the rule itself for this purpose, then it seems we must point to something about the pupils own understanding of the rule. That is, as Dummett says when describing the case of teaching addition, we need to show them that if they go on in a different way to the way we would, they have done something which they, were they more enlightened, would recognize as going on in the wrong way. Either this, or we must say that they have misunderstood from the start. And since we cannot say when even so called non-deviant pupils have mastered the rule, we cannot say what actually constitutes following the particular rule in question and no other, except that there are some answers we feel disinclined to accept. Wittgenstein's examples of individual deviant rule-followers

are, according to Stroud, aimed at getting us to recognize just this point. That even in the case of non-deviants, we cannot say either when they have mastered the rule, nor what constitutes such mastery except that their practice is in agreement with ours. This mounts a challenge to traditional conventionalist accounts because it undermines the appeal to understanding that forms the central plank of such accounts.

The conventionalist appeals to the idea of properly understanding the rule, which is based upon recognizing that its meaning is objective; which is to say something like the following: the rule I am now following is to be applied *thus*, and this is an objective fact about *that* rule, and if I have understood that rule aright, then of course it will be apparent to me that I should apply it *thus*, and if it is not so apparent then of course I have misunderstood. When such considerations are projected by analogy onto the deviant rule-follower it is concluded that they *must* have misunderstood, and that continuing in such a way *must* be a matter of perversity, stupidity, or idiocy.

Again, to be sure I am clear here. We are not talking about a case where what is involved is the pupils understanding wrongly. In the situation we are dealing with here the pupil does not see how there could be any way to go on other than the way that they are going on. For the pupil to be going on wrongly, they must realize at some point that there are alternative ways to go on that are just as acceptable. Again, we can think of Kripke's example of quaddition. Imagine a world in which everyone but you behaved like the bizarre skeptic. It's not that the pupil is mistaken, because at the point we are talking about, even the notion of mistake is not clear anymore. In such a situation, do we really have the right to tell the pupil their way of going on is wrong? And on what basis? If it is not that of the rule itself, as Platonism would have us believe, then what could we possibly point to to justify our censuring of them? Particularly when we cannot point to anything to justify our assertion that we are correct in our way of going on.

Finally, we need to get clear about something and to deal with a line of argument that at first might seem to be open to the conventionalist, and by which they may avoid the threat

the rule-following considerations pose to their concept of understanding, particularly as explicated through the foregoing example. This line of argument proceeds as follows. The conventionalist might admit that it is not possible for anyone else to judge even remotely accurately when the pupil has either finally come to understand or definitely begun to misunderstand the rule; but surely this does not prevent the pupil from so doing?

Again, let me make what I mean here clear. The standard conventionalist, as we have seen, disputes the independent existence of rules that somehow intimate to us how to go on, and hence the claim that our ways of following the rule are the only possible ones; but they maintain over against this that there is an objective fact of the matter as to what counts as going on in the correct way in so far as we are concerned: that the meaning of mathematical and logical truth is objective. Usually, the standard conventionalist grounds such an account in something called "understanding". The conception of understanding involved here shares much with its cognate phrase "grasp of". The image usually conjured up by this conventionalist use of the word "understanding" is of ones being in possession of something, like knowing a secret that might not be known to others. Now, the conventionalist claims above might be colloquially expressed by, "when you've got it, you've *got* it"—in which the stress laid of the second "got" has a double function in signaling that "got it" is the appropriate phrase to use to describe the situation, *and* in referring itself to the situation at hand, of your being in possession of something or other. You might almost say that because the conventionalist believes that the meanings of mathematical and logical truths are objective, that even though they *don't* have independent existence, in so far as our understanding or grasp of them is concerned we might as well say that they *do*.

This state of affairs will not do. As we shall see in more detail later, and as Hilary Putnam points out, the above view is a "weird" one (Putnam 1979 p. 430). Interestingly enough, this view, which also holds that the meanings of all logical and mathematical truths are in fact the same since they assert absolutely nothing about the world, is the view

implied by much of what Wittgenstein has to say about logic in *Tractatus Logico-Philosophicus*. The point is that the standard conventionalist view seems to require that we could, as it were, sense a contradiction as immanent if we adopted a set of rules that would in the long run turn out to contradict each other. Either that, or we are committed with this conventionalist to admit that if at any time in the future there is cause to doubt that such a contradiction *does not* exist, then we must be prepared to admit that we have misunderstood all along. Obviously the standard conventionalist does not favor such logical nihilism, and in general it is for this reason that they claim that what they call "understanding" when present gives us access to the one uniquely uncontradictory set of rules. The problem is that they still want to maintain that *we make the rules up*, but how could we as *finite beings* appropriately grasp whether or not a *rule with infinite future applications*, will end up contradicting other rules we already have that themselves have *infinite future applications* that we are not yet aware of. Again, Dummett seems to be thinking of just this sort of thing when he says, "in deciding to regard a form of words as necessary...we have a responsibility to the sense we have already given to the words of which the statement is composed" (Dummett 1966 p. 438). But how is this a responsibility we can properly discharge when the rule that is stipulated by those words has an infinite number of future applications?

It is at the above point in that radical conventionalism comes into play; picking up the pieces of the shattered notion of understanding, and substituting in its place the notion of decision; whether taken communally or individually. But we have already seen in Chapter 1 the difficulties such a suggestion presents, and without any other clearly spelled out alternative, in many respects we are where Dummett left us in the beginning.

The proof is supposed to have the effect of persuading us, inducing us, to count such-and-such a form of words as unassailably true, or to exclude such-and-such a form of words from our language. It seems quite unclear how the proof accomplishes this remarkable feat. (Dummett 1966 p. 430)

And it is perhaps appropriate that this is so. It has already been mentioned that Stroud thinks that Dummett has not read Wittgenstein's examples aright and that that is why he has not been able to get beyond his attribution of radical conventionalism.

Despite this what we have brought to light so far, *does* have important implications for conventionalism, in so far as there are some things that it definitely *does not* imply. In the first place, one thing that is not implied by what has been said so far, is that radical conventionalism is correct. Secondly, nothing has thus far been said to imply that we cannot yet give an account of the objectivity of mathematics that amounts to more than just a shallow, or merely linguistic account. Thirdly, all that *has* been so far shown is that the claim that there are any *independently existing* rules that on the basis of their existence are capable of showing us how to go on is of no help, since the problem does not lie there. The weak link in the Platonist's account has been identified; the weakness of Platonist accounts is not the abstract nature of the rules they supposedly claim to exist, it is in the claim advanced in tandem to this one that we are able to somehow *understand* these rules such that we can never go wrong in applying them. Standard conventionalism is weakened by the foregoing considerations because it shares in this concept of understanding. And as the above paragraphs show, if we cannot say when it is that someone has mastered the technique of following the rule, then we cannot say what understanding consists in other than agreement in practice.

Earlier I said of what Wittgenstein tells us about the individual deviant, the untutorable pupil, that all of this tells us something about the grammar of the words “understand” and “misunderstand”. And while so far we have seen that the notion of understanding that is central to standard conventionalist accounts of rule-following turns out to be bankrupt, nothing has been suggested to fill the breach. Radical conventionalism seems to be the only genuine alternative to Platonism, but this has a cost. It threatens the objectivity of mathematical truth. This should be clear from what we had to say about radical conventionalism in Section 3 of the last chapter. To argue against Platonism successfully,

it *seems* that you must also argue against the objective meaning of mathematical and logical truths. If the independent existence of rules of inference cannot make any difference to how we go on, because we can never guarantee that *anyone* has understood them properly, then the only reasonable alternative seems to be that we *decide* what counts as going on in the right way. This strikes me as a rather hasty conclusion to draw, and in the next chapter and the last we will hopefully see why, but as a hint we have the following quote from *Philosophical Investigations* §182.

The criteria which we accept for 'fitting', 'being able to', 'understanding', are much more complicated than might appear at first sight. That is, the game with these words, their employment in the linguistic intercourse that is carried on by their means, is more involved—the roles of these words in our language other—than we are tempted to think.

Chapter 4

Wittgenstein's Unintelligible Woodsellers.

§ 1. Introduction.

As I have said, Stroud identifies two main types of examples that Wittgenstein uses to shed light on his insight about the nature of rules and rule-following. The first type, which Dummett focuses on, are examples of a deviant in our own midst. The second type of example usually involves a parable of some sort about an alien society who do things radically differently to the way that we do, and among whom we would seem to be the outsider.¹⁷ In connection with the idea that Wittgenstein's examples have a heuristic value I want to examine more closely the scenario of the society of woodsellers.

Stroud begins from the idea that the examples are, at least *prima facie* very plausible. According to Stroud this is supposed to be so, because we are supposed to actually imagine what would come to pass if things were the way the example describes them being. One example of this second type is the society of wood-sellers who sell wood not by the cubic measure, or weight, as we would, but by the amount of area it covers when laid on the ground. The greater the area a pile of wood covers the greater the price, and Wittgenstein seems to assume that this is their *only* criterion for determining the price of the pile.

Before going any further, I think we ought to take a look at the woodsellers scenario as Wittgenstein describes it himself. Stroud does not do this and I think that his account is weaker for it. While he foregrounds the most important conclusion to be drawn from a careful examination of the this scenario, that Wittgenstein is trying to point something out

¹⁷ The only example of this sort that really stands out is the woodsellers scenario from *Remarks on the Foundations of Mathematics*. But similar scenarios are in use to make a similar point to the one I take the woodsellers scenario to be making here in *Philosophical Investigations* in particular see §207-8, where a similar society is mentioned, and compare the tribe mentioned at §200 who play chess by yelling and stamping.

to us by highlighting the ultimate unintelligibility of the woodsellers practices, he perhaps misses some important clues as to the finer points of Wittgenstein's strategy in arguing this way. Two of these clues are that the point to be demonstrated, the conclusion which Stroud rightly draws from the example, once again involves exposition by example, and that the strategy which Wittgenstein here employs in a negative or, without contemporary connotations, deconstructive way, is very similar to the language-games strategy that he employs in a positive or constructive way at the beginning of *Philosophical*

Investigations. Again, I think that we can perhaps learn something from the way this strategy is employed in a negative way that might be a valuable lesson when examining how it is positively employed: that the primary end sought is exposition via example, not elaboration or description of what is actually there, because there is nothing actually there but what is described in the description of the example itself. There are no such things as language-games existing side by side with human activities, but descriptions of examples of many human activities can be very usefully compared to game-like activities, or seen as language-games.

§ 2. The Woodsellers Scenario.

143. ...We teach someone to build a house; and at the same time how he is to obtain a sufficient quantity of material, boards, say; and for this purpose a technique of calculation. The technique of calculation is part of the technique of house-building.

People pile up logs and sell them, the piles are measured with a ruler, the measurements of length, breadth and height are multiplied together, and what comes out is the number of pence which have to be asked and given. They do not know 'why' it happens like this; they simply do it like this: that is how it is done.—Do these people not calculate?

147. Suppose I had said: those people pay for wood on the *ground of calculation*; they accept a calculation as proof that they have to pay so much.—Well, that is simply a description of their procedure (of their behavior).

148. Those people—we should say—sell timber by cubic measure—but are they right in doing so? Wouldn't it be more correct to sell it by weight—or by the time that it took to fell the timber—or by the labor of felling measured by the age and strength of the woodsman? And why should they not hand it over for a price which is independent of all this: each buyer pays the same however much he takes (they have found it possible to live like that). And is there anything to be said against simply giving the wood away?

149. Very well; but what if they piled the timber in heaps of arbitrary, varying height and then sold it at a price proportionate to the area covered by the piles?

And what if they even justified this with the words: “Of course, if your buy more timber, your must pay more”?

150. How could I shew them that—as I should say—you don't really buy more wood if your buy a pile covering a bigger area?—I should, for instance, take a pile which was small by their ideas and, by laying the logs around, change it into a ‘big’ one. This *might* convince them—but perhaps they would say: “Yes, now it's a *lot* of wood and costs more”—and that would be the end of the matter.—We should presumably say in this case: they simply do not mean the same by “a lot of wood” and “a little wood” as we do; and they have a quite different system of payment from us. (RFM, I)

Let us make some observations. The “technique of calculation” used to find out how much wood is needed to build the house is first shown in its familiar context of “the technique of house-building”. Secondly, a situation is described in which people are selling wood in what would seem to be the usual way, but it is said that they are not able to say why they do things that way, they just seem to accept that this is the way it is done. (The parallel with our own case, in particular in situations of ordinary language use, ought to be pretty obvious.) The question of whether or not these people calculate is briefly posed, and answer that they do in fact buy and sell wood “*on the ground of calculation*” is suggested. It is also noted that this is not so much an answer to the question as a description of what they do. (Again, it is possible to draw a parallel with our own case and how we would typically answer the question of whether we in the same

situation calculate; of course we do, we use calculation in buying and selling wood to decide how much is to be exchanged for what sum of money, because this just is what it is to buy and sell wood.) Other ways of deciding how much the wood is worth are then suggested, including how much it weighs, time taken to fell, and labor of felling. (To see that these are reasonable think of why you might want to pay less for two cubic metres of balsa wood than for the same amount of iron bark, and why it might be thought that rare timbers should actually be worth more, for example the timber of the trees of the Arctic tundra, or of the Borneo rain forest, or ebony.) Finally the ideas of just giving the wood away or selling it for a price not determined by the amount are suggested. (These suggestions don't seem to have any corresponding examples in our culture, but perhaps in the cultures of some older human societies where property was not individually owned we could find such examples, and again this is reasonable given this context. The example which involves no correlation between quantity and price, is a little harder to contextualize, but perhaps we might find something like this in extremely affluent societies, hence, perhaps, Wittgenstein's own aside at the end.)¹⁸ Then Wittgenstein hits us with the bomb-shell, in the last example price and quantity *are* correlated, but in a way that by our lights does not make sense and can never do so. What can we learn from this?

Well for one thing, we should notice that right up until we get to the last society we are able, by progressive stretches of the imagination, to understand why these people might do what they do. It is only with the last society that things become really strange. It is important to remember here that not just one society is mentioned, several are, and each is different, though of course they do all seem to be in some way related, and that is important. We will return to this methodological point a little later, right now we ought to address the question that is probably foremost on our lips: what is the point of this example?

¹⁸ There are reports of people buying loaves of bread with whole wheel-barrows full of money during the heady days of hyper-inflation in Weimar Germany.

§ 3. The Point of the Examples: The Development of the Critique.

As we saw earlier, Stroud argues that the main targets of Wittgenstein's writings on mathematics, meaning, and necessity were Platonists like the early Russell and Frege. This led Stroud to argue that Dummett's reading of Wittgenstein as an attack upon conventionalism was wrong. Stroud went on to say that while Wittgenstein's writings can be read as posing problems for conventionalist accounts of meaning, and hence for logical and mathematical necessity as meaning-based concepts, this does not signal a rejection of conventionalism on Wittgenstein's part. Again, while the alternative that Wittgenstein seems to have had in mind might not have emerged clearly from what he did say, what does emerge from what he had to say gives us good reason to think that, at least, he did not favor what has been so far labeled radical, or full-blooded conventionalism. What remains for Stroud to do is to show that Wittgenstein gives a genuine alternative to the radical conventionalism the rule-following considerations seem to imply, while not simply rehashing the conventionalism which those considerations about following rules do so much to undermine. It seems to me that a first step in this direction can be made by examining what Stroud tells us about what we might call the general strategy of Wittgenstein's argument, a second by examining what Stroud takes to be the main conclusion that is revealed through this strategy, and a third by examining how this conclusion is foregrounded using the technique of exposition via example (which I take to be one of Wittgenstein's favorite technical devices). Finally, we shall see how this fits within Wittgenstein's general philosophical methodology.

§ 4. The Point of the Examples: The Strategy of Argument.

I should briefly say what I mean by 'strategy' as I am using it here. I understand the strategy of an argument to be the general outline of how the argument is to progress if it is to succeed. Thus the strategy will specify which steps are to be taken, which objectives are to be secured, and at least roughly, in what order. Since many techniques of argument are subtle, it is not always possible to give the exact order of progression of argument

steps without also describing the primary technique of argument. For the moment we will concern ourselves with the strategy.

These two sorts of examples can seem like, and I believe that they are supposed to be, a logical development of the one point. First of all, Wittgenstein points out that we have trouble justifying to someone who goes on differently why going in on one way rather than another when following a rule is what you should do, as exposed through the various restatements of the deviant pupil example. Secondly, he points out that it is at least possible to conceive of going on differently to be the rule rather than the exception as it is for us. And this is where the alien society examples like the woodsellers scenario come into their own.

As I have mentioned, Stroud does not actually reproduce the woodsellers scenario in full, but gives a brief description and finishes it by quoting §150 from *Remarks on the Foundations of Mathematics*, which I will re-quote for convenience sake.

150. How could I shew them that—as I should say—you don't really buy more wood if your buy a pile covering a bigger area?—I should, for instance, take a pile which was small by their ideas and, by laying the logs around, change it into a 'big' one. This *might* convince them—but perhaps they would say: “Yes, now it's a *lot* of wood and costs more”—and that would be the end of the matter. (RFM, I)

Stroud then goes on:

This case is analogous to that of trying to get the deviant pupil to see that the next step after “1000” is really “1002.” But can we describe what these people do as “selling wood in the wrong way”? Is it a way whose “incorrectness” we could point out to them? And surely it is not logically impossible for there to be such people: the example does not contain a hidden contradiction. (Stroud 1966 pp. 483-4)

So what is the point of this comparison?

Well, we have already shown what sort of implications the rule-following considerations have for both the Platonism which Stroud says was to be their actual target, and for the conventionalism that actually shares similar misgivings about Platonism. While the conventionalist argues against the Platonist that there is no ontological basis for the objectivity of mathematics, and thus no absolute normativity, the rule-following considerations seem to present a case against there being any normative dimension to our understanding of any rules at all. Whereas conventionalism argues that right and wrong in going on to follow a rule originate in our *understanding* of the rule, rather than its existential nature, the rule-following considerations seem to imply that even understanding cannot provide an appropriate basis for the normativity of rules. The purpose of the above comparison should now be a little more obvious, but let me restate this just to be clear.

Platonism proposes that there are rules "out there" which we read off when acting in a way described as following the rule. Conventionalism objects, saying that there are no rules "out there" to read off, and that what we do is unpack an initial "understanding" of the rule into its many applications. Both views hold mathematics to be objective, but for different reasons. The rule-following considerations imply that there is no sense in which one can be said to *understand* a rule such that it is possible to say that the rule that is grasped is that particular rule and that rule only, except in so far as it is possible to follow a rule without thinking about it, to follow a rule *blindly*. The upshot of these considerations is therefore the contention that one can only say which rule was being followed in hindsight, by pointing to the actual practice of the rule-follower. On the basis of these considerations it seems impossible to say, along with the conventionalist, that there can be any *absolutely* correct or incorrect way to follow the rule, nor, that there can be any correct or incorrect way to follow the rule supposedly based upon the *understanding* of the rule which is apparently being followed. It seems that the deviant pupil cannot therefore be said either to have gotten it wrong, in anything other than the most shallow sense of the term, or that the pupil has in fact misunderstood the rule in

question. It follows that it would be entirely possible for there to be fully consistent systems of rules that were incommensurable with our own in the most fundamental ways, for example that there could be a logic from which you could infer "p" from "if p then q" and "q". And furthermore, as we shall see, it *logically* follows from this that since we cannot distinguish, except with hindsight, between activities which constitute following *our* system of rules, and those that constitute following *some other* system of rules which is fundamentally logically incommensurable with our own, that it is not possible to say what does and does not count as correctly following the rule *now* by way of appeal to logic, or to what it makes sense to do.¹⁹ Hence the woodsellers scenario does not contain a contradiction, and we cannot therefore describe their way of selling wood as *wrong on any logical grounds*, even though we find it utterly unintelligible in terms of our own system of rules, including our own system of logic. To put this another way, the point logically developed here is that logic cannot itself arbitrate as to what actually counts as logical and what as illogical. (This is, I believe, what Wittgenstein is talking about when he says that "agreement...in judgments" seems to "abolish logic" even though it does not in fact do this (PI §242).)

§ 5. Digression II: Runabout Inference.

I want now to briefly discuss an article by A. N. Prior. The interesting thing about this article is that it addresses the very issue discussed above, but with specific relevance to formal logic. This treatment of the weakness of conventionalist accounts of logic is not the only other such account. As noted by many commentators on this issue, the work of both N. Goodman and W. V. O. Quine also deals with this issue, or at least the central parts of it.²⁰ Apart from the fact that the Prior's treatment of this issue relates it

¹⁹ My talk of "systems" of rules here is merely a device. While, we might describe our rules for all sorts of things as forming a system, we do not mean that they form a system in a complete sense. They are not like mathematical models of systems, for example, which are complete. The myriad of different sorts of rules that we have to regulate our various practices are both heterogeneous and discontinuous; they are not all of the same type, and they are not all infinitely extendable. the situation is rather like the following; the five-sector-flow model of economies putatively represents the flow of money between the various sectors of the economy, but in practice people break the rules of exchange, lose money, and get swindled.

²⁰ For these references see, Stroud 1966 & 1969, Kripke 1982 pp. 55-8, and Ben-Menahem pp. 120-1.

specifically to formal logic, it is recommended by two other features: the paper is short, and it deals specifically with the extension of a supposedly consistent set of axioms which yields a new inconsistent set where no contradiction is involved in extending the original set of axioms thus.

In his paper "The Run About Inference-Ticket", Prior describes the conventionalist account of the meanings of logical connectives as follows.

It is sometimes alleged that there are inferences whose validity arises solely from the meanings of certain expressions occurring in them. The precise technicalities employed are not important, but let us say that such inferences, any such there be, are analytically valid. (Prior 1967 pp. 129)

He goes on to give as an example the explanation of the validity of conjunctive inference based solely upon the meaning of the expression "and" which is used to conjoin any two statements "P" and "Q" to give the composite "P and Q". He then points out that a doubt might be raised as to whether there will always be a statement "R" such that it can be inferred from any two statements "P" and "Q" by conjoining them with the connective "and", that is, that there is a pair of statements "P" and "Q" such that it would *not* be possible to infer a third, "R", from them and from it once again infer "P" and "Q". He says that this doubt is dismissed by defenders of the above conventionalist thesis as reflecting "an old superstitious view" about the type of meaning that such analytically valid inferential connectives must have, which asserts that their meaning is independent of how we employ them: in short, Platonism (Prior 1967 pp. 129-30).

Prior then makes the point that "in this sense of 'analytically valid' any statement whatever may be inferred, in an analytically valid way, from any other" (Prior 1967 p. 130). He then goes on to give the example of inferring "2 and 2 are 5" from "2 and 2 are 4" in the following way.

2 and 2 are 4.

Therefore, 2 and 2 are 4 tonk 2 and 2 are 5.

Therefore, 2 and 2 are 5. (Prior 1967 p. 130)

"Tonk" we are told, is to be defined by the stipulation of the following two rules:

- (i) from any statement P we can infer any statement formed by joining P to any statement Q by 'tonk' (which compound statement we hereafter describe as 'the statement P-tonk-Q') and that (ii) from any 'contonktive' statement P-tonk-Q we can infer the contained statement Q. (Prior 1967 p. 130)

The point made here is similar to the one made above. If the meanings of logical connectives, and hence what counts as analytically valid, are solely a matter of convention, then given appropriately framed conventions we can extend our system of logical axioms to include one which defines the connective "tonk" as above. We simply frame a new convention about the meaning of the term concerned. This is of course the radical conventionalist solution to the dilemma the rule-following considerations pose for standard conventionalism. But there is more. Prior has defined the new connective "tonk" by appeal to the same notion of "analytically valid" that is appealed to by definitions of familiar connectives like "and". And this notion of "analytically valid", as said, is itself supposedly based solely upon the meanings of such words as "and" as we use them. Since this tells us nothing about how to use new words like "tonk", we are free to frame new conventions of the sort that give it the above meaning. But a little stretch of the imagination should show that we do not even have to introduce a new word like "tonk" (Prior 1967 p. 130).

In the beginning of his paper Prior mentions that validity of inferences involving "and" stems solely from the meaning of "and" as we use it "in the purely conjunctive sense" as distinct from "its colloquial use to mean 'and then'" (Prior 1967 p. 129). But as Prior's own introduction of tonk demonstrates, nothing prevents us from dovetailing new connectives onto our existing system of logical axioms so long as the inferences involving them can be shown to be analytically valid in the appropriate sense, that is, by appealing solely to their meaning. This being so, similarly, nothing prevents us from redefining

existing connectives so long as inferences involving them can be shown to be analytically valid in the appropriate sense. We could, for example, redefine "and" to include the colloquial use to mean "and then", thus bringing temporality into logic. Which for convenience we will call "and*". All we need to do is to show that the validity of inferences involving "and*" stems solely from the conventional meaning of "and*" as we use it. (This could easily be done by stipulating that from "P and* Q" only "Q" can be inferred, because Q can never come temporally before P. In other words, we eliminate inferences like "P and* Q", "Q", therefore "P", but retain those like "P and* Q", "P", therefore "Q".)

§ 6. The Point of the Examples: The Unintelligibility Conclusion.

We can now move on to examine what conclusion Wittgenstein's strategy of argument yields according to Stroud. As the digression involving Prior's paper is supposed to show, the point that the examples of both types logically develop is as follows. Appeal to the *understanding* of a convention cannot demarcate *a priori* what counts as an acceptable new convention, nor what counts as a legitimate application of a rule that is constituted by an existing convention. As such there is *no logical* basis for our following rules in the way that we do, and so *no logical reason* why the conventions that constitute those rules could not be otherwise than they are, even to the point of allowing modifications to our set of logical axioms with implications that are incommensurate with those axioms as they stand. Despite this, it seems impossible to make any intelligible sense out of examples like the woodsellors scenario, and so something must constrain our stipulation of new conventions which is not our understanding of those conventions that we already have. An alternative needs to be suggested to standard conventionalism.

This logical development is basically the core of Wittgenstein's critique of both Platonism and conventionalism, the problem is of course that the results of this critique seem to be incoherent. Or at least that is how it has seemed. The alternative apparently offered by the critique—Dummett's radical conventionalism—says that what counts as

going on in the same way is a matter of decision, that going on as we do is a matter of making an arbitrary stab in the dark. But the whole point of the critique of conventionalism is to point out that the conventionalist needs to explain why this does not happen; and they need to do it without the benefit of the Platonic imagery of us reading off how to go on.

One thing implied by saying that we have adopted, or are following, a convention is that there are alternatives which we could adopt in its place. But in the case of writing "1002" right after "1000" there appear to be no alternatives open to us. It seems impossible to understand how we could "adopt the convention" that writing "998, 1000, 1004,..." is going on in the same way, or taking steps of the same size. Surely if writing "998, 1000, 1002,..." is not taking steps of the same size, then nothing is.

I have been trying to suggest so far that for Wittgenstein such "alternatives" are not inconceivable or unimaginable because they involve or lead to a logical contradiction...there is no logical contradiction involved in supposing that someone might agree with us in all uses of words or in all steps of a proof up to the present, and that he should now accept something different from what we all accept as the conclusion... (Stroud 1966 p. 484-5)

It is worth remembering in this connection that most versions of the deviant pupil example begin by describing how the pupil has been taught how to add, and the errors in their counting, or calculations don't occur until they have surpassed the scope of the calculations etc. covered in their training. It seems that if we trust them to get from "1" to "100", then we ought to trust them to get from "100" to "1000", and so on, but when they reach "1000" things start to go wrong. But this is fair way along the series of natural numbers. Why pick this point rather than the transition from "100" to "102", for example?

Stroud has an answer to this, and it takes the form of the conclusion that he thinks we ought to draw from the point that is logically developed above. He says:

I think the initial intelligibility and strength of Wittgenstein's examples derive from their being severely isolated or restricted. We think we can understand

and accept them as representing genuine alternatives only because the wider-reaching consequences of counting, calculating, and so forth, in these deviant ways are not brought out explicitly. When we try to trace out the implications of behaving like that consistently and quite generally, our understanding of the alleged possibilities diminishes. I suspect that this would happen with most, if not all, of Wittgenstein's examples, but I do not need to prove this in general, since if my interpretation is right these examples will fulfill their intended role whether or not this point holds. (Stroud 1966 p. 488)

For another thing the fact that the only criterion these people seem to have for selling wood is the amount of ground it covers seems to lead to all sorts of conceptual difficulties. To save space I will just quote from Stroud here:

Surely they [the wood sellers] would have to believe that a one-by-six-inch board all of a sudden increased its size or quantity when it was turned from resting on its one-inch edge to resting on its six-inch side.²¹ And what would the relation between quantity and weight possibly be for such people? A man could buy as much wood as he could possibly lift, only to find, upon dropping it, that he had just lifted more than he could possibly lift. Or is there more wood, but the same weight?...Also, it would be possible for a house that is twice as large as another built on exactly the same plan to contain much less wood. How much wood is brought need have no connection with how much wood is needed for building the house. And so on. (Stroud 1966 p. 488)

And in the end here we come back to the very place Wittgenstein's original example starts from: "[t]he technique of calculation is part of the technique of house-building" (RFM, I, §143).

Dummett has simply not followed Wittgenstein's examples through far enough. He has embellished them but he has failed to notice what this shows. He has recognized that radical conventionalism is incoherent, but he has not realized that this shows us

²¹ Notice even the words used here, Stroud speaks of a "one-inch edge" and a "six-inch side", because by our lights the thinner side is so thin in proportion as to qualify as an "edge" for our descriptive purposes, though this in itself implies that the surface area of the one-inch side is less than that of the six-inch side. To see the relevance of this contrast; "turned from resting on its one-inch side to resting on its six-inch edge."

something about conventionalism generally. He has not asked why the examples turn out to be unintelligible, and why Wittgenstein would be putting forward a view he seems so obviously to have noted the incoherence of himself. Stroud also has an answer to this.

The reason for this progressive decrease in intelligibility, I think, is that the attempt to get a clearer understanding of what it would be like to be one of these people and to live in their world inevitably leads us to abandon more and more of our own familiar world and the ways of thinking about it upon which our understanding rests. The more successful we are in projecting ourselves into such a world, the less we will have left in terms of which we can find it intelligible. In trying to understand these alleged possibilities, we constantly come across more and more difficulties, more and more questions which must be answered before we can understand them. But this is not to say that we do not understand them because they are "meaningless" or "contradictory," or because what they purport to represent is "logically impossible." (Stroud 1966 p. 488-9)

I said that the move from deviant-in-our-midst examples to alien society examples seemed to involve the logical development of a point. With the individual deviant we find ourselves in the curious situation of casting about for a criterion that we normally don't explicitly appeal to, and on finding nothing more solid than convention at first, our legs turn to jelly. And when we ask ourselves whether or not there are intelligible alternative ways of understanding as a result of adopting alternative conventions, it seems that we would have to say "Yes". So it seems we would have to say that its not impossible to think the logically impossible, but this seems wrong, if anything is impossible to think it ought to be the logically impossible. Hence Dummett's claim about the incoherence of radical conventionalism. And this is right. The point as, Stroud sees it, is that this paradoxical situation only arises because the results of embellishing the examples have come off in the wrong way. In fact the point being developed here is not just a logical one, but also a socio-historical one, or even perhaps a factual one. In relation to this Stroud quotes the following passage from *Remarks on the Foundations of Mathematics*:

What we are supplying are really remarks on the natural history of man: not curiosities however, but rather observations on facts which no-one has doubted, and which have only gone unremarked because they are always before our eyes. (RFM, I, §142)

According to Stroud if we take this sort of lesson away from Wittgenstein's examples than we have understood them aright. What initially presents as the development of some logical point, actually also turns out to be a series of observations couched in examples that make certain of these observations more obvious than they otherwise would be. To get the full impact of these observations we are to embellish the examples enough to see that all they are meant to show is that there is no logical reason why we could not do things otherwise than as we do. But in so embellishing them we have come to see that though its not logically impossible for things to be done in these different ways, it is not possible to actually compare the different ways of doing them, because there just is no ground for comparison. Hence, although we can see that there may be such unintelligible alternatives, we cannot really understand them. As such the examples are both illustrative and conceptually enlightening, and Stroud continues on from the paragraph quoted immediately above to draw out the explicit implications of this for radical conventionalism in light of the fact that Wittgenstein's intended target is Platonism.

Wittgenstein's examples are intended to oppose Platonism by showing that calculating, counting, inferring, and so forth, might have been done differently. But this implies no more than that the inhabitants of the earth might have engaged in those practices in accordance with rules which are different from those we actually follow. It is in that sense a contingent fact that calculating, inferring, and so forth, are carried out in the ways that they are—just as it is a contingent fact that there is such a thing as calculating or inferring at all. But we can understand and acknowledge the contingency of this fact, and hence the possibility of different ways of calculating, and so forth, without understanding what those different ways might have been. If so, then it does not follow that those rules by which calculating, and so forth, might have been carried out constitute a set of genuine alternatives open to us among which we could choose, or even among which we could have

chosen...this does not imply that we are free to put whatever we like after "1000" when given the instructions "Add 2," or that our deciding to put "1002" is what makes that the correct step. Consequently, Wittgenstein's examples do not commit him to a "radical conventionalism" in Dummett's sense. In trying to explain more fully why he is not committed to this I will return to the sense in which he can be called a "conventionalist." (Stroud 1966 p. 489-90)

I want to return to the theme that Stroud ends on here, "the sense in which [Wittgenstein] can be called a 'conventionalist'", in the next chapter, where we will take up Ben-Menahem's suggestion that Wittgenstein held to a descriptive conventionalism. For the moment I think it will suffice to say that I take this passage to be Stroud's strongest rejection of Dummett's attribution of radical conventionalism to Wittgenstein, and to present concisely the central thrust of his argument. The rejection of Platonism does not imply radical conventionalism, and since Platonism is the target of Wittgenstein's examples the attribution of radical conventionalism must be upheld on other grounds. Embellishment of the examples shows such grounds are lacking, and hence some form of conventionalism ought to be attributed to Wittgenstein as his default position. This "default position" will be elaborated in the following chapter.

§ 7. Wittgenstein's Philosophical Methodology.

In this concluding section we will wrap up what Stroud has to say, and return to the point raised at the end of Section 1 of this chapter by relating observations made in the foregoing chapters to three of Wittgenstein's better known technical devices: form of life, language-games, and family-resemblances. We start with "form of life" because this is where Stroud's exposition of Wittgenstein's views ultimately ends up, and a brief consideration of what he has to say will bring us back to our point of origin, the phenomena of logical compulsion.

According to Stroud, Wittgenstein thought that the source of logical compulsion is our form of life. "Our form of life" refers to the myriad of contingent facts about ourselves

and our world which make up what might be called "our natural history" (Stroud 1966 pp. 494-5). Such facts include facts about the reliability of our memories, the fact that most people who have been taught to add don't go on in ways different to the customary ones, that marks put on paper don't generally spontaneously change of their own accord, and so on. These are in particular all the sorts of facts that Wittgenstein imagines not obtaining when framing his various examples of deviants failing to follow the rules in the usual way. These are also the facts that we typically rely upon to make those examples themselves intelligible to us. By selectively highlighting the contingency of all of these facts he at one and the same time shows that they are all individually contingent and that, in a sense, taken as a whole they are not. This is not supposed to mean that taken as a whole they are necessary, but that taken as a whole they amount to something that we cannot ignore without falling into unintelligibility. In connection with this, Stroud says that what he is proposing here explains "what would otherwise be a puzzling distinction which Wittgenstein makes in a well-known passage" (Stroud 1966 p. 492).

I am not saying: if such-and-such facts of nature were different people would have different concepts (in the sense of a hypothesis). But; if anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean not realizing something that we realize—then let him imagine certain very general facts of nature to different from what we are used to, and the formation of concepts different from the usual ones will become intelligible to him. (PI II p. 230)

About this passage Stroud says:

The point of Wittgenstein's examples...is only to show that our having the concepts and practices we have is dependent upon certain facts which might not have obtained. They show only that "the formation of concepts different to the usual ones" is intelligible to us; but it does not follow from this that those concepts themselves are intelligible to us. And since the intelligibility of alternative concepts and practices is required by the thesis of radical conventionalism...that thesis is not borne out by Wittgenstein's examples. (Stroud 1966 p. 493)

Stroud goes on to describe these facts as "examples of what Wittgenstein calls the 'physical', 'psychological', and 'physiological' facts which make activities such as calculating possible" (Stroud 1966 p. 491; RFM, VII, §18). It is in virtue of the obtaining of these facts that, according to Stroud, that we find it "natural" to go on in the way that we do.

So understanding the rule in the way we do depends upon such things as finding it natural to go on to "1002" right after "1000." That we take just the step we do here is a contingent fact, but it is not the result of a decision; it is not a convention to which there are alternatives among which we could choose. And that we share any such "judgments" at all (whatever they might be) is also a contingent fact, but without this agreement there would be no understanding of any rules at all. (Stroud 1966 p. 491)

We will return to the importance of our sharing "judgments" of the sort that Stroud mentions here in the next chapter. For the moment, we can again begin to create a substructure for work to be done later—predominantly in the last section of the next chapter—by relating some of these observations to the use of language-games as a technical device.

In the beginning of *Philosophical Investigations* Wittgenstein builds up a series of primitive language-games into a progressively more sophisticated all-encompassing language-game of building (PI §2, §§6-8, §10, §§16-21 & §41, many other passages refer, but most of the construction of the language-game is over by this point.). The activity is constructive—if you'll excuse the pun—but the larger language-game is not the sum of its parts, because the smaller language-games that make it up are heterogeneous. They are not all reducible to some simple formula they all use words in different ways, even where it is the same word (phonetically speaking) being used. The methodology employed here seems similarly imaginative, though, if I can use the word without too many contemporary connotations, the activity is deconstructive. We are encouraged to build up a picture of a progressively stranger and stranger alien society, we move

progressively into stranger and stranger lands. And at some point, we can no longer understand what is going on around us. There are two stages to this. The first stage involves the stripping away of more and more familiar aspects of the activities of buying and selling wood, more and more of the stage setting is removed. First the purpose of acquiring the wood drops out of the picture, then the idea of measuring wood in the usual way drops out, then the idea of pricing drops out. In the end though, we could still admit, if grudgingly, that what is going on *could* be called selling wood. The second stage involves recombining almost all of these progressively removed elements, but correlated differently to the way in which we correlate them. With the result being that *we can only see* the aliens using one criterion as being relevant to selling the wood, how much ground it covers, and this situation is unintelligible for us.

Finally, and only very briefly, we can relate these considerations to another of Wittgenstein's technical concepts, family-resemblance. At several points in his later work, and in particular in *Philosophical Investigations*, Wittgenstein describes the relationships between different language-games, or different regions of language as being like a series of family-resemblances (PI §§65-67, §§162-164 & §179). There are three ways in which this comparison is relevant to the observations of the foregoing chapters and to the strategy of argument used to draw what I have been calling "the unintelligibility conclusion" from them. The first of these relates to the first type of example.

As we have seen type one examples typically involve a situation in which someone is attempting to master a skill or technique of some kind, and in most cases this is a skill or technique that will allow them to participate in some language-game or other. We have also seen that Wittgenstein takes great care to be thorough in his descriptions of these situations, to the point of following through the process of either teaching or learning from its beginnings through to the point at which the main point of the example is raised, when the pupil deviates from standard practice. As we saw in §143 and §185, Wittgenstein describes the full range of transitions from guiding the pupils hand through

to getting them to, at least in the first place, master the technique for using the formation rule "0, n, 2n, 3n..." (PI §143 & §185). Again, a similarly thorough description is involved in his development of the builders language, and the discussion of reading, both in *Philosophical Investigations* and *The Blue and Brown Books* (BB pp. 77-83 & pp. 117-125; PI §2, §§6-10, §§16-21, §41 & §§156-71). It seems to me that at each point of learning-transition in these language-games—from guided hand to independently continuing the series, from "pillar" and "block" to "b slab (hands red sample over)"—involves a step of recognition by the pupil or builder, a step of recognition that involves recognizing a family-resemblance.

Secondly, I have pointed out that it seems to me that Wittgenstein makes use of language-games in both "constructive" and "deconstructive" ways. As an example of the deconstructive use of language-games as a technical device I pointed out that the woodsellers scenario involves the description of a series of language-games, each less and less like the foregoing, until the language-game that ends up being described is ultimately unintelligible to us. I have said that it seems to me that the deconstructive use of language-games involves stripping away of more and more familiar aspects of the activities of buying and selling wood; is this not just the gradual distancing of our current language-game from the foregoing ones by removing more and more family-resemblances? Isn't it the case that each language-game resembles the first less and less because more and more of the features it and first share are being removed? Couldn't we see the change from the first language-game to the most recent as being like genealogical decent where a set of once dominant features gradually fades, like the gradual assimilation of one set of so called "racial characteristics" into those of the more populous "racial" group? It seems to me that we could, and that such a description would be very accurate.

Finally, it strikes me that if we compare these examples we can see a pattern in the methodology. In all these cases Wittgenstein seems concerned to build up what seem to be intuitively appealing and plausible scenarios, while continually drawing our attention to

the ways in which the uses of language being introduced are different. In other words, the heterogeneity of the various language-games introduced is stressed, yet the story being told loses none of its plausibility. What I mean to suggest by all of these observations is that the family-resemblances between language-games are being foregrounded here. Since family-resemblances are not necessarily transitive or symmetrical, but form a complex pattern of linkages—A resembles B in features 1 and 2, B resembles C in features 2 and 3, D resembles A and C in features 1 and 4, and so on—they are unlikely to be the sort of thing we could compile an exhaustive list of. So highlighting them can only come about through the technique of exposition by example, the very same technique which Stroud shows us is at work in Wittgenstein's writings on logical necessity.

Chapter 5

Constructivism and Meaning.

§. 1 Introduction.

We have now seen how Stroud has responded to Dummett. A few things should be borne in mind. In the first place Stroud's response does not meet Dummett's attribution of radical conventionalism to Wittgenstein "head on", we could perhaps say that rather it side swipes it, but no matter the metaphor the point is that Stroud's response is indirect. The importance of this fact will I hope become apparent in the following but we can probably say something about it now. Conventionalism is a view about how statements, in particular logical and mathematical ones, get their meaning. It is not actually a view about *what they mean to us*.²² The debate that has ensued over Wittgenstein's position on the issue of logical necessity seems to have gone on in ignorance of this fact. And I think that the reason why can be found in the history of the debate itself. Putnam in the paper we will analyze below makes the point that the really strange view about logical necessity is the one held by the positivist's, conventionalisms strongest proponents. On this view the meanings of logical truths were all, strictly speaking, the same (Putnam 1979 p. 430). That is, because all logical truths were empty tautologies, and meaning was to be given by the truth conditions of statements, they strictly speaking had the same truth conditions and so the same meaning. This view contains the seed of the problem because it runs two important things together; how statements of any kind get to be meaningful at all, and

²² Ben-Menahem has drawn attention to this as an aspect of the descriptive conventionalism she attributes to Wittgenstein. When describing the differences between description and explanation to illustrate what is distinctive about descriptive conventionalism, she identifies the primary difference between explanation and description as follows. "First, explanations, even of a human activity, are offered from an external point of view, while descriptions are internal to the 'form of life' that harbors the described activity... We must not take Wittgenstein to mean that there can be no explanation of human behavior. His argument is that such explanations, though sometimes feasible, fail to identify a meaning the persons involved would recognize as the meaning of their activity" (Ben-Menahem 1998 p. 109). We will have reason to come back to this idea later on, but I should say now, in anticipation, that she thinks that this point—along with the idea that what Wittgenstein thinks we are describing here are internal relations—actually speaks against the idea that he was an anti-realist.

what those statements mean to us. I will not say too much more about this except to say that I think that if we move away from a truth conditions account of meaning this conflation is less easily made, and that Stroud's emphasis on form of life, is on this view very much headed in the right direction. We now move on to a brief recapitulation and clarification of the story so far.

§ 2. Conventionalism.

A typical but succinct statement of the general thesis of standard conventionalism is to be found in a paper by Hans Hahn entitled “Conventionalism”. Contrasting it with what he calls “[t]he old conception of logic” as “the account of the most universal properties of things”, Hahn characterizes the conventionalist view as follows:

Our thesis, on the contrary, asserts: logic does not by any means treat of the totality of things, it does not treat of objects at all but *only of our way of speaking about objects*; logic is first generated by language. The certainty and universal validity, or better, the irrefutability of a proposition of logic derives just from the fact that it says nothing about objects of any kind. (Hahn 1968 pp. 45-6)

On this view, as Hilary Putnam tells us, the statements which encapsulate logical and mathematical truths are seen not as statements of fact, but as directions about what are to count as acceptable uses of statements of fact. As Putnam puts it in his paper “Analyticity and Apriority: Beyond Wittgenstein and Quine”, and I like his formulation because it is clear, such a “statement is not a “description” of any fact, but a “rule of description”—that is, a directive to the effect that cases in which we *seem* to add two things to two things to get five, or whatever, are to be explained away” (Putnam 1979 p. 424). This is borne out by what Hahn has to say about someone asserting, say, “I added two and two and got five”. “The point is—it is best to express it in the language which any card player is familiar with—that the man has revoked: he has violated the rules in accordance with which we want to speak, and I shall refuse to speak with him any longer” (Hahn 1968 p. 49).

Now, while other conventionalists might not be quite as brash as Hahn, we can see the general point. It is because the statements that encapsulate mathematical and logical truths are directives, rules of description for how we should speak about objects, that treating them as statements of fact whose negation constitutes something we can make sense of, is not acceptable. When we affirm the negation of a statement encapsulating a logical or mathematical truth, then we are simply babbling nonsense. We are violating the rules of talk, and we ought not to be surprised if Hahn thumbs his nose at us, turns swiftly on his heel, and marches away.

We can briefly state the positive thesis advanced by conventionalism this way. Language is constituted by conventions, and these conventions are of two sorts, those that correlate statements with the facts that they stand for, and those that stipulate what are to count as acceptable ways of deploying or using statements that stand for facts. Conventions of the second sort therefore give us statements that are directives about how to speak about objects rather than being about objects themselves. These statements are sometimes referred to as “meaning postulates”, but they might also be described as “directives”, “rules of description”, or as being about our way of speaking about objects. They are “rules of description” in so far as they tell us what to count as acceptable ways to use statements that describe facts, or that are about objects.

It should be obvious that even in logical terms the above division of statements into factual statements and rules of description, is not exhaustive. There are many logical, and perhaps more importantly, mathematical statements which would fall into neither category. These are logical and mathematical statements that are not factual statements, but which are also not rules of description or directives. The meanings of these statements are not said to be conventional, but rather to be the *consequences* of the meanings of conventional statements. That is, it is held that the meanings of these statements can be generated in their entirety from the factual statements and rules of description. In other words, *we did not have to adopt a new convention to give them a meaning*, their meaning

was there all along, so to speak. As Putnam says. “In a terminology employed by other philosophers, the statement is *analytic*” (Putnam 1979 p. 424). An example of such an “analytic” statement would be a theorem of logic or mathematics generated from a set of axioms like the Peano Axioms whose meanings are conventional. For those not familiar with them:

P1. 0 is a number

P2. The successor of any number is a number

P3. No two numbers have the same successor

P4. 0 is not the successor of any number

P5. If P is a property such that (a) 0 has the property P , and (b) whenever a number n has the property P , then the successor of n also has the property P , then every number has the property P . (Hempel 1949 p. 226)

In a paper entitled “The nature of Mathematical Truth”, Carl Hempel states these five axioms and shows how given them we can derive the sequence of natural numbers and the basic arithmetic functions. The following is just a taste.

The construction of elementary arithmetic on this basis begins with the definition of the various natural numbers. 1 is defined as the successor of 0, or briefly as $0'$; 2 as $1'$, 3 as $2'$, and so on. By virtue of P2, this process can be continued indefinitely; because of P3 (in combination with P5), it never leads back to one of the numbers previously defined, and in view of P4, it does not lead back to 0 either. (Hempel 1949 p. 226)

§ 3. Wittgenstein's Difficulty.

Putnam tells us that Wittgenstein's problem with this view is as follows:

Now, the thesis that every theorem of mathematics is either true by convention (a Meaning Postulate in Carnap's sense, or a “rule of description” in Wittgenstein's) or else a *consequence* of statements that are true by convention has often been advanced as an *epistemologically explanatory thesis*, but it cannot really explain the truth of the theorems of mathematics (*other* than the ones in the finite set that are *directly* “true by convention”) at all, for a reason pointed out by both Wittgenstein and Quine: namely, *it takes*

logic to derive the consequences from the conventions. The “exciting” thesis that logic is true by convention reduces to the unexciting claim that *logic is true by conventions plus logic*. No real advance has been made. (Putnam 1979 p. 424)

So the meanings of statements like the one that Hempel offers above cannot really be consequences *only* of the meanings of Peano’s axioms as conventionally defined and nothing else. Even if we remove the various statements which frame his derivation of the sequence of natural numbers.

We can think of it in terms of logic in this way. We cannot draw the theorems of logic as consequences of statements that stipulate the rules of logical inference without making use of the notion of inference we are attempting to define by demonstrating that to infer is just to draw *those* particular consequences. Such a derivation *depends upon* the idea that the theorems are in fact consequences of the conventional meanings of the statements stipulating the rules of inference. Again, if you were to say, “you’re not following the convention, and don’t understand the meaning it gives the statement, if you do not draw the consequences we do from it”, then you can hardly also say, “what counts as a consequence of the convention is a matter of what the statement means”. It is almost a chicken and egg problem.²³

In his paper “Wittgenstein’s Philosophy of Mathematics”, Dummett considers Wittgenstein’s problem with the standard conventionalist account of logical necessity and what he considers to be Wittgenstein’s rejection of that account. According to Dummett, this leaves Wittgenstein with the view that we are in fact free to choose what we can draw as consequences from those statements whose meanings are given by convention. According to Dummett, who has called this position “full-blooded conventionalism”, Wittgenstein thought that in fact the meanings of all statements are conventional, that the

²³ If to understand what “potential of” means is just to draw “q” as a consequence of the conventional meanings of “if p then q” and “p”, then you cannot say that “potential of” is to be defined by the fact that the application of *modus ponens* to “if p then q” and “p” yields “q”.

consequences are not in fact consequences at all, and that what counts as appropriate logical inference is a matter of *decision, not recognition* (Dummett 1966 p. 425).

According to Dummett, this view is incoherent, because we would have to accept that someone who did not draw the same consequences as we did, both did and did not understand what the conventionally defined statements meant.

In his paper “Wittgenstein and Logical Necessity”, Barry Stroud mounted a challenge to Dummett’s interpretation of Wittgenstein. Stroud argued that according to Wittgenstein logical necessity was grounded in our “form of life” (Stroud 1966 pp. 89-95). As Putnam describes the situation:

Barry Stroud pointed out that the position Dummett calls, “radical conventionalism” cannot possibly be Wittgenstein’s. A convention, in the literal sense, is something we can legislate either way. Wittgenstein does not anywhere say or suggest that the mathematician proving a theorem is *legislating* that it shall be a theorem...Basing himself on a good deal of textual evidence, Stroud suggested that Wittgenstein’s position was that it is not *convention* or *legislation* but our *forms of life* (i.e., our human nature as determined by our biological-plus-cultural-history) that cause us to accept certain proofs *as* proofs. (Putnam 1979 pp. 424-5)

He goes on to describe this interpretation as a “revision of Dummett’s rather than a total rejection of it”, because “if *either* Dummett *or* Stroud is right, then Wittgenstein is claiming that mathematical truth and necessity *arise in us*”(Putnam 1979 p. 425).

§ 4. The Consistency Objection.

According to Putnam such views are subject to a fatal objection, which we will follow him in calling the “consistency objection” (Putnam 1979 p. 425), and he believes that for this reason Stroud’s response “while correct as a response to Dummett’s interpretation, does not speak to the real philosophical point Dummett was making” (Putnam 1979 p. 425). The consistency objection can briefly be stated as follows. Since the consistency of any set of conventionally defined “*meaning determinations*” as Putnam calls them, is a

property of that set itself, rather than of the conventions that define them, it follows that what count as the consequences of that set of meaning determinations cannot itself be a matter of convention. Using the Peano Axioms as an example, Putnam puts this in a striking way when he says “[o]ur nature, our forms of life, etc., may explain why we *accept* the Peano axioms *as opposed to some other consistent set*; but our nature cannot possibly make an *inconsistent* set of axioms *true*. And consistency is an *objective mathematical fact*, not an *empirical fact*” (Putnam 1979 p. 425). Consistency is an objective property of a set of conventionally defined meaning determinations, and is therefore not something which our decisions about which consequences to draw from them can confer upon them.

There is a throw-away response to this which Putnam briefly considers, that we would be able to operate with an inconsistent set of meaning determinations so long as the inconsistency never strikes us. He says that this response does not speak to the objection. I’m not sure if this is entirely right. Putnam goes on to look at what he considers to be Wittgenstein’s “*real* response to the consistency objection”, which he says “goes to the very depths of his philosophy”, but I think that perhaps the throw-away response is actually a significant sign of something deeper (Putnam 1979 p. 425). More on this later.

§ 5. Putnam on Following a Rule.

According to Putnam, Wittgenstein’s “*real* response” to the consistency objection is to be found in what he has to say about what it is to follow a rule. We will start with Putnam’s own “un-Wittgensteinian” statement of the problem and then move on to a couple of other accounts (Putnam 1979 p. 426). Putnam summarizes these considerations thus.

Whatever introspectable *signs* there may be that I have of [a] concept, whatever mental presentations I am able to call up in connection with the concept, cannot specify the *content* of the concept...if two species in two possible worlds...have the same mental signs in connection with the

expression “add one,” it is still possible that their *practice* might diverge; and it is the practice, as Wittgenstein shows, that fixes the *interpretation*; signs do not interpret themselves—not even mental signs. (Putnam 1979 p. 426)

What we should remember here is that these considerations about rules are constitutive rather than, say, epistemic. The upshot is basically that there is no way to justify the claim that I am following any rule at all. And this can be generalized to the point of saying that unless what constitutes a rule can also be said to interpret the rule for us then all we can point to is our actual practice as a guide to how to interpret the rule. So even in the case of Putnam’s “mental signs”, if these cannot be said to be self-interpreting, or to specify their own *content* then they cannot be what constitutes our understanding of the rule. As Wittgenstein himself says “no course of action could be determined by a rule, because every course of action can be made out to accord with the rule” (PI §201).

According to Putnam, these considerations are relevant for mathematics and logic in two respects. Firstly, since our actual practice with (the number of times we have applied) mathematical and logical rules is only finite, and will only ever be so, we cannot say that there is any unique sequence that actually constitutes the sequence of natural numbers *for us*. Our practice does not single out any one interpretation of the rule, because there are an infinite number of interpretations of the rule, all of which agree with our actual finite applications of the rule (our practice so far) but diverge from one another after that. It would seem that we cannot master even the technique of counting, because if this were so our practice ought to determine one unique sequence as the correct one, but it does not and cannot. Secondly, it follows from this that for sufficiently general mathematical statements we are required to say that the most we can ever have for them are “proof-conditions”, which means that in the case of these statements the law of excluded middle does not hold. The statement “the theorem “ $0 = 1$ ” would never occur in Peano Arithmetic” would be an example, according to the considerations above, all we can say is that up to some point, to use Putnam’s example up to 10^{20} places, we have proofs that show that the theorem “ $0 = 1$ ” does not occur (Putnam 1979 p. 426).

§ 6. Others on Following a Rule.

The literature on Wittgenstein's notion of following a rule is voluminous, and it would take a great deal of space to review even a significant fraction of it. Even so, it seems that many recognize Saul Kripke's exposition of Wittgenstein's account as being as important as it is controversial. Again, to cover this controversy adequately would take great space, but we can confine ourselves to a few sparse remarks. Kripke has given significant attention to what he believes is the underlying argument in Wittgenstein's *Philosophical Investigations* with particular attention to what he has called "the Wittgensteinian paradox" (Kripke 1982 p. 7). This paradox is a constitutive paradox about meaning. The problem is as follows: there is according to Kripke no fact about myself to which I can point to justify my claim to be following any rule at all if I am challenged to do so. The point is not that ordinarily I do have to provide such justification, but that were I asked for such, none could be given (Kripke 1982 p. 21). We don't even ordinarily think of ourselves as following a rule when we are doing so, at least not in so far as we think to ourselves "now I am following the rule for constructing a +2 series", but we do describe what we do as following the rule for constructing a +2 series, and a conception of us as following some particular rule and no other comes along with this way of describing things. The point is that this description is apt to lead us astray because we are led to think of ourselves as following some independently specifiable rule, and this in turn leads us into thinking that there is something special about us when we are indeed properly described as following the rule. What is special about us is that there is some fact about us that shows that we are indeed following a rule, and the independently specified rule at that (Kripke 1982 pp. 53-4). The problem is that careful introspection will show that there is *nothing* special about us when we are properly described as following a rule (Kripke 1982 pp. 41-51 & p. 56). And this is because there is *no fact* about us which shows that we are following some particular rule. Indeed there is nothing about us that shows that we are following any rule at all (Kripke 1982 pp. 7-22 & p.55). The paradox, as we shall see, is that this lack of a fact about us that shows that we are following any rule at all

makes no difference. That there is nothing special or different about us when we are following a rule seems to undermine our understanding of what it is to follow a rule. “Surely,” we will say, “there *must* be *something* different about us when we are following the rule! How else are we to tell when we are and when we are not?” The answer according to Kripke, and which he attributes to Wittgenstein, is that we cannot, at least individually, tell when we are following the rule and when we are not. Hence there could be no private language in which we could specify either first-personally *or* individually that we are following any rule at all (Kripke 1982 pp. 60-2, 69-75).²⁴

Kripke calls the solution given to the paradox a “sceptical solution” and says that this is the solution that Wittgenstein gives us to the Wittgensteinian paradox in the form of the famous private language argument (Kripke 1982 pp. 78-109). This argument, which is actually an exposition by example of a collection of arguments, shows the impossibility of a private (first-personal) language by showing that there must be rules for the deployment of its signs, and that such rules are not available to us privately (first-personally). The idea is that meaning is use, and there cannot be use without rules, and there cannot be rules that are privately grasped. Hence all language is public, and all meaning public, and any sense in which we grasp rules is also public. Language is by nature public, and so the problem which the paradox seems to pose is dissolved; it *does not* arise for us, because it *cannot* arise for us, because we cannot follow rules privately. The idea that we could ever follow rules privately is just a mistaken conception generated by a misunderstanding of language itself; we take our description of ourselves too seriously, and we think that when we *say* we are following a rule we are describing just that, ourselves following the rule, when *really* we are not describing anything of the sort (Kripke 1982 pp. 75-8). If

²⁴ I use “first-personally” here to allude to the fact that what makes the paradox paradoxical is that we as individuals cannot tell whether we are following any rule *because* we cannot from our first-person perspective establish introspectively that there is anything different about us when we are properly described as following the rule. There are no “mental signs”, as Putnam calls them, that point to some independently specifiable rule that we are following *because* thought signs don’t point to anything, we only describe them as if they did; it is for this reason that we describe the situation in terms of the idea that there could not be a private language, a language spoken both by *and* to the first-person, and only them.

we are describing anything at all, we are only describing what we are all inclined to do at that point, and even that might be going too far.

In his book *Wittgenstein on Meaning: An Interpretation and Evaluation* Colin McGinn challenges Kripke's interpretation of Wittgenstein, mainly by reading the "private" in "private language argument" as "first-personal" (McGinn 1984 pp. 59-92). According to McGinn, while the considerations about rules show that our conception of the notions of grasping or understanding a rule are confused, they do not show that we are never in a position to be justified in making the claim that we are following some rule or other. We can be justified individually, if not first-personally. We *can* according to McGinn point to our own past actions and see these as showing that we are following some particular rule or other, the point is that we *cannot* further justify this claim by pointing to anything about us that was different or special when we were following the rule in question. Thus I could follow a rule individually so long as my performance was public. The best way to describe the difference between McGinn and Kripke might be to say that while McGinn thinks that all rule-following—and hence all language—must be public, Kripke thinks that all rule-following must be both public and *shared*.

Though much more could be said, I think we have enough here to be going on with. We might just note that one of the main aspects of the controversy over Kripke's exposition is his claim that Wittgenstein would have rejected a dispositionalist solution to the paradox. Kripke thinks that any dispositionalist solution suffers from similar defects to what he generally calls the "straight solution": that being a solution which supposedly offers some account of what is different about us when we are following a rule; that we are in some special mental state or other, for example (Kripke 1982 pp. 22-37). Later we shall see that this aspect has some bearing on how Putnam spells out what he takes to be Wittgenstein's "*real* response" to the consistency objection. Putnam, as we know, favors

talk in terms of dispositions, and more precisely *de facto* dispositions, which I take it are distinguished from counter-factual dispositions or some such.²⁵

§ 7. Which is Wittgenstein's View?

Putnam tells us that Wittgenstein's view, as spelt out here "does not work", but he says that to see that this is so we must first "resolve an ambiguity in the view" (Putnam 1979 p. 427). Which he describes using the example of *modus ponens*.

even so simple an operation as *modus ponens* is not "fixed" once and for all by our mental representation of the operation; it is a our actual "unpacking" of the mental representation in action, our *de facto* dispositions which determine what we *mean* by "ponential of." (Putnam 1979 p. 427)

He goes about doing so with his usual clarity by describing two possible "scenarios" of how this "unpacking" might turn out in the case of using *modus ponens* to derive theorems from Peano's Axioms.

Scenario (1): Given a putative proof one checks it by going down line by line, verifying that each line with *ax* next to it is an axiom, and that each line with two-numbers (n) (m) next to it is the ponential of the lines numbered (n), (m) respectively. If the last line is " $1 = 0$," one announces "Peano Arithmetic has turned out to be inconsistent." *Scenario (2)*: Given a putative proof one proceeds as in scenario (1) *except that if any* line is " $1 = 0$ " (or anything verifiably false by just elementary calculation and truth-functional logic), then one *modifies what counts as ponential* so that the line in question is said not to be the ponential of the relevant lines (n), (m). (Putnam 1979 p. 427)

Putnam goes on to say that only if our actual dispositions were as scenario (2) describes them would it be the case that Peano Arithmetic was "consistent in the absolute sense, and that this consistency would *arise from us*, be explained by our nature in a clear

²⁵ It is curious that Putnam actually feels that he needs to talk about *de facto* dispositions. Nevertheless, the very idea of a *de facto* disposition strikes me as idle, not for what *it* is, but for what it implies: that there are some dispositions, which are in fact not dispositions, or perhaps are not dispositions which we in fact have. If "disposition" means "tendency" or "inclination" then it seems a little strange to talk of dispositions that we are not inclined to have. I don't mean dispositions we are not inclined to have now, but dispositions we would presumably never be inclined to have, which is the only sense I can make of the notion of something to contrast with *de facto* disposition.

sense” (Putnam 1979 p. 427). He maintains against this that our actual dispositions are as scenario (1) describes, and while our interpretation of the Peano axioms is “fixed only by our dispositions” the “*consistency of Peano Arithmetic is still not an artifact of this dispositionally fixed interpretation*” (Putnam 1979 p. 427).

It is important to understand what is and is not being denied here. Putnam says that he is “*not denying that mathematical truth is “perspectival” in the sense of depending for its very content upon our actual existential natures and dispositions*”, but only that it is “*an artifact of the way we use the words that Peano Arithmetic is...consistent*” (Putnam 1979 p. 427). He introduces a second example to make this clear, he says that the truth of the judgment that there is a Mountain Ash on his property depends upon our form of life, but that does not mean that the fact that there is a Mountain Ash on his property is merely an artifact of the way we use the words. In his own words “it is not a truth that is *explained* by facts about human nature; it does not *arise from us*” (Putnam 1979 p. 428). We will return to this idea later, but what I take from this is that Putnam is *actually* more concerned to ward off *radical constructivism* than he is to ward off radical conventionalism. Radical conventionalism is a view about the meanings of logical and mathematical statements, radical constructivism is a metaphysical view about the nature of reality.

So the situation is roughly this. The claim that the *content* of our concepts, be they mental signs or whatever, is only “unpacked” by our *de facto* dispositions, can be seen as ambiguous. It can be taken, as in scenario (2) to show that therefore the content of our concepts is *constructed* by way of conventions. That the consistency of Peano Arithmetic is just an artifact of the way we use the words. And it can be taken, as in scenario (1) to show that the content of our concepts is only *fixed* by way of conventions, which means that the consistency of Peano Arithmetic does not have to be an artifact of the way we use the words.

It seems to me that what Putnam has made here is a negative claim. It does not follow from the considerations about rules that the consistency of Peano Arithmetic *must* be an artifact of the way we use the words. But I am not sure that this is the same as the positive claim that it *is not* such an artifact. But the way is not barred to asserting this, the rule-following considerations *don't* show that we have to accept that the consistency of Peano Arithmetic *is* an artifact of the way we use the words.

We can now see to some extent how the controversy about Kripke's claim that Wittgenstein would have rejected a dispositionalist solution to the paradox has a bearing on Putnam's account. Although Kripke's and McGinn's books were both written well after Putnam's article, we can see that even Putnam anticipated some of the problems that are raised by differing interpretations of the rule-following considerations and what they show. In particular the putative ambiguity that Putnam is talking about here seems to me to be the basis of the difference between these two interpretations. But again, more later.

§ 8. Conventionalism or Constructivism?

The view that Putnam seems in favor of in contrast to the one he ascribes to Wittgenstein might be described as a form of cautious constructivism.²⁶ A constructivist holds that we have a significant role to play in determining what the world is like. A moderate or cautious constructivist will probably hold that we are always in a position to interpret things in more than one way. Therefore at least some parts of, say, mathematics will be arbitrarily constructed within predetermined limits. We have at least *some choices* about what will count as part of mathematics, but these are always limited by our previous choices. Our interpretation of the world is just that, interpretation not creation. A more radical constructivist holds that we have a much greater degree of freedom, that much more of mathematics is arbitrarily constructed and that our choices are only predetermined, or prescribed, within fairly specific limits. Perhaps it is best to contrast the two views over the issue of consistency we have just been discussing. A moderate

²⁶ I am indebted to Dr John Burgess for suggesting this point to me.

constructivist would deny that we could make *any* inconsistent set of axioms consistent by creating a new convention. A radical constructivist would say that only *some* inconsistent sets of axioms cannot be made consistent by convention; for example, only those that are explicitly strictly contradictory. Finally, constructivism can take an extreme form which would hold that there are no inconsistent sets of axioms if “inconsistent” means something like, “we can’t make this set of axioms work,” for an extreme constructivist consistency is always absolute in Putnam’s sense.²⁷

Now, the view that the rule-following considerations do not rule out our asserting that the consistency of Peano Arithmetic *is not* an artifact of the way we use the words, seems to me to be a form of cautious or moderate constructivism. It is constructivist in so far as it recognizes that the only thing that *fixes* the interpretation of the rule are our *de facto* dispositions, as Putnam calls them. It is cautious in holding that this *does not* mean that how we follow *at least some* rules is entirely a matter of our *form of life* or our “biological-plus-cultural-history” as Putnam calls it; and hence that, for example, “mathematical truth and necessity *arise in us*” (Putnam 1979 p. 425).

The alternative view that Putnam, and perhaps Dummett, attribute to Wittgenstein—that the rule-following considerations *do* rule out our asserting that the consistency of Peano Arithmetic *is not* an artifact of the way we use the words—seems to me to be a form of extreme constructivism. As mentioned above, for an extreme constructivist, consistency is always absolute in Putnam’s sense, and this fits with the idea that our *de facto* dispositions actually *construct* or *create* the rule rather than just *fix* its interpretation. We might also say that our interpretation of the rule *is* the rule on this account.

²⁷ There is a good chance that these classifications of the various sorts of constructivism map straight onto a similar classification made by Michael Dummett in his paper “Some Reflections On Wittgenstein and Logical Necessity” (Dummett 1992 p. 446). The names are the same and perhaps this is the same for the views they refer to.

We are now in a position to draw some important observations about the case Putnam is making against Wittgensteinians. Putnam says in the beginning of his paper he aims to show that “not even the most sophisticated of these 'Wittgensteinian' views is tenable” (Putnam 1979 p. 423). “Wittgensteinian” for Putnam refers to both Dummett’s and Stroud’s interpretations of Wittgenstein’s views, and in particular to what their interpretations seem to share, *the idea that logical necessity arises in us*.

In the first place, we ought to pause to step back. The initial debate between Dummett and Stroud was over whether or not Wittgenstein was a radical conventionalist about logical necessity, whether or not he held that the necessity of logical or mathematical truths was just a matter of our treating them as unassailable. According to Dummett this was so, but in the end this view turns out to be incoherent. According to Stroud, the view does turn out to be incoherent, but Wittgenstein did not hold that view, though he wanted to draw our attention to the fact that neither it nor the alternative standard conventionalist view are completely satisfactory. What remains is the need to refine standard conventionalism along some sort of contextualist lines. Putnam has weighed into this discussion by arguing that none of these alternatives is satisfactory given the rule-following considerations. He thinks that these considerations point toward Wittgenstein advocating a form of extreme constructivism which flies in the face of the objectivity of mathematics.

In the first chapter I went to some pains to argue that most of the problems Dummett raises for Wittgenstein’s account of logical necessity are not problems rooted in the philosophy of mathematics, but are problems from the philosophy of meaning. I think that that point needs to be re-stressed. What Putnam is giving us here is a problem from metaphysics. He argues that the rule-following considerations *do not* rule out the objectivity of mathematics, but that Wittgenstein held that they did, and he points to Stroud’s emendation of Dummett’s attribution of radical conventionalism as corroborating this. The point is, the objectivity of mathematics can be understood *either* as it pertains to

metaphysics *or* as it pertains to the philosophy of meaning. In the case of the latter it is a matter of how we as subjective beings come to understand, if indeed we ever do, the objective meaning of mathematical statements. In the case of the former it is a matter of what the world is like and how it impacts upon us, of what sorts of things there are “out there”, and of what sorts of changes we can actually make to them. It seems to me Putnam introduces considerations about the objectivity of mathematics as it pertains to metaphysics in order to draw morals for how it pertains to the philosophy of meaning.

§ 9. From Conventionalism to Constructivism.

In my opinion the origin of the transition identified above, from radical conventionalism to extreme constructivism actually lies in what Stroud has to say in defense of Wittgenstein contra Dummett. Stroud introduces the notion of “form of life” into the debate about whether Wittgenstein was a radical conventionalist, and in so doing he introduces the idea that human nature plays a role in our accepting that certain statements are logically or mathematically necessary. Or in other words, he introduces the idea that human nature in some way determines or at least affects our concepts of logical and mathematical necessity.

Dummett is concerned to show that Wittgenstein’s view is incoherent, he wants to show this as first and foremost a purely logical problem. For Dummett the view he attributes to Wittgenstein is *logically incoherent*. Stroud considers Dummett to be basing this attribution on a misreading of Wittgenstein’s examples and counters thus.

I have been trying to suggest so far that for Wittgenstein such “alternatives” are not inconceivable or unimaginable because they involve or lead to a logical contradiction...there is no logical contradiction involved in supposing that someone might agree with us in all uses of words or in all steps of a proof up to the present, and that he should now accept something different from what we all accept as the conclusion...(Stroud 1966 pp. 484-5)

What Stroud does is to import the idea that for Wittgenstein the way we are, our human nature, means that we can't have concepts of logical and mathematical necessity other than the ones that we do. This, in effect, does not address the charge of logical incoherence "head on", but sidesteps it with the assertion that there are some empirical facts about us that make the possibility of our having different concepts of logical and mathematical necessity unintelligible for us. The charge of radical conventionalism is answered with the claim that it doesn't fit with our form of life.

Putnam has taken Stroud's counter to Dummett to be the claim that mathematical and logical truth is somehow generated out of our form of life. This is a move from the idea that from within our form of life, as a result of our nature, we are not able find alternative concepts of mathematical and logical truth intelligible, to the idea that our *actual* concepts of mathematical and logical truth are a result of our form of life.

It is at this point that the transition is complete, from Dummett's original claim that Wittgenstein's examples commit him to a radical conventionalism that is logically incoherent, we have moved to the claim that they commit him to a form of constructivism so radical that it constitutes, in Putnam's words, "an astounding philosophical claim" (Putnam 1979 p. 425).

It seems to me that the point of transition is in fact the rule-following considerations themselves, these considerations can be read as showing that it must be the case that we *construct* rules as we go along, that we (as a community) somehow make them up. Putnam is right in his claim that these considerations can also be read in such a way as to not rule out the claim that this is not so, i.e. the claim that we don't construct rules as we go along. Thus Putnam makes the claim that Wittgenstein's rule-following considerations yield an ambiguous view. The view in question might either be taken to be an extreme constructivist view, or a more moderate constructivist view. (These are views that flow from the rule-following considerations; they are not the view, if any, stated in those considerations, which are mainly critical remarks and examples of the type we have

examined.) Both interpretations of the view which flows from the considerations need to account for the observed regularity of our practices; the normativeness of rules. The extreme constructivist view builds in the normativeness of rules in the same way that it builds in absolute consistency. According to such a view there is *only one way the rule is followed*, as opposed to the idea that *there is only one way to follow the rule*. According to Putnam such a view implies that our *de facto* dispositions would be like those described in his scenario (2), where the meaning of “potential of” is changed so that from our point of view Peano Arithmetic remains consistent. Now, this is not radical *conventionalism*, if anything it is some sort of very radical *constructivism*, the important difference is that our *de facto* dispositions can only be as described in scenario (2), if say, in Putnam’s words, Peano Arithmetic is “consistent in the absolute sense” for us (Putnam 1979 p.427). This means that the consistency of Peano Arithmetic would have to depend not just upon our present dispositions, but upon our whole history. For us to be disposed as scenario (2) describes, we would have not only to understand something different by “potential of”, but also by “consistent”, and “infer”, and so on. We would need to have a radically different way of going about the world. In short, as Stroud maintains, we would have to be constituted in a way, which by our current lights, is an unintelligible way for us to be.

This transition also has another source, it is also motivated by the way in which the examples are viewed. Both Putnam and Dummett view Wittgenstein’s examples in radically different fashion to the way that Stroud views them. And it seems that what Putnam draws from them are implications which are even more radical than Dummett’s charge of logical incoherence. Let me explain.

In his paper on Wittgenstein’s view, Dummett uses the example of someone who goes on differently to the way that we do in continuing a series. Stroud also uses this example, but he adds the case of the woodsellors, which involves a whole society of persons who arrange their activities in a way that we do not, and which from our perspective is

irrational, or illogical. These examples are positive. What I mean by this is that they talk about how things might become different if we were to find out such-and-such, or if we were to be exposed to such-and-such: they talk about how things might be different in a way that implies that we might find they are different elsewhere or sometime in the future. Putnam's example is different, it implies that we might find the *we have been wrong* in the past, that we might *now* come upon a contradiction and have to fix it. That we might perhaps have started with a, unbeknown to us, inconsistent set of axioms, and realizing now that we cannot work with them as inconsistent we change them to suit what seems to us to be a "smooth and natural" extension of our past practice (Stroud 1966 p. 496). This way of putting things is negative, in my usage, the example is not about how we might see things change from now on, or more precisely, *fail* to see how they have changed. Rather, it is about how we might in the future *fail* to accept that what we committed ourselves to in the past was wrong. This "backward looking" aspect of Putnam's example is the aspect I have described as "negative".

Failing to accept what we might have committed ourselves to in the past is more difficult to understand, but it gets closer to the actual state-of-play, and it appropriately illustrates the intractability of the Kripke's Wittgensteinian paradox about following rules. The question is whether or not it follows from this that Wittgenstein was some form of extreme constructivist. If the answer that the private language argument gives us *is* a constructivist answer, then we need to say just how radical such a constructivism would have to be. According to Putnam Wittgenstein took his rule-following considerations to show that we constructed rules, and hence mathematical and logical truth, and mathematical and logical facts, as we went along. He claims that such a view cannot work because our *de facto* dispositions would have to be like those spelled out in scenario (2) and it is a matter of empirical fact that they are not. This not only gives support to Dummett's attribution of radical conventionalism, it also makes a different point, it makes the point that extreme constructivism *must be untrue*, and this is different to the point that

radical conventionalism is *logically incoherent*. The two criticisms are related, but they are not the *same*.

§ 10. Empirical Facts and Mathematical Facts.

Once we have recognized the difference between attributing some sort of radical *conventionalism* to Wittgenstein and attributing some sort of very radical or extreme *constructivism* to him, it becomes possible to see clearly why Putnam takes his objection to the Wittgensteinian view to be successful. As I have already tried to indicate I do not believe that this response to Stroud actually addresses what he has to say. I believe that in fact Putnam only directly addresses what Stroud has to say in digression.

In the section of his paper entitled “Another Wittgensteinian Move” Putnam tells us that Wittgenstein might also have had the following move in mind.

One might hold that it is a presupposition of, say, “ $2 + 2 = 4$,” that we shall never *meet* a situation we would *count* as a counterexample (this is an *empirical* fact); and one might claim that the appearance of a “factual” element in the statement “ $2 + 2 = 4$ ” arises from *confusing* the mathematical assertion (which has *no* factual content, it is claimed) with the empirical assertion first mentioned. (Putnam 1979 p. 428)

In short this move comes to the following, its just an empirical fact about us that we do not accept anything as a counterexample to a logical truth, the statement of that truth is just a reminder of that empirical fact and not a replacement for it. In my opinion this is *much* closer to the view that Stroud attributes to Wittgenstein when he says things like this about continuing the +2 series:

That we take just the step we do here is a contingent fact, but it is not the result of a decision; it is not a convention to which there are alternatives among which we could choose. And that we share any such “judgments” at all (whatever they might be) is a also a contingent fact, but without this agreement there would be no understanding of any rules at all. (Stroud 1966 p. 491)

I believe that the “judgments” that Stroud mentions here are similar to the judgment that anything which seems to be a counterexample to, say, “ $2 + 2 = 4$ ” is to be explained away by saying something was added or something subtracted without our noticing.²⁸ The paragraph that immediately follows the one above in Stroud’s paper is a quote from §242, of *Philosophical Investigations*, this passage is one of a set of three from §§240-2 that I think are relevant here, not only because they make clear what Stroud is getting at, but because it seems to me that they anticipate just the sort of problem Putnam has with Wittgenstein’s account.

240. Disputes do not break out (among mathematicians, say) over the question whether a rule has been obeyed or not. People do not come to blows over it, for example. That is part of the framework on which the working of our language is based (for example, in giving descriptions)

241. “So you are saying that human agreement decides what is true and what is false?”—It is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions but in form of life.

242. If language is to be a means of communication there must be agreement not only in definitions but also (queer as it may sound) in judgments. This seems to abolish logic, but does not do so.—It is one thing to describe methods of measurement, and another to obtain and state results of measurement. But what we call “measuring” is partly determined by a certain constancy in results of measurement.

The above sequence of passages seems to fairly clearly indicate that in fact what Wittgenstein is talking about here are just the sorts of “judgments” that would be at the same time described and expressed by statements of *empirical* facts of the sort that

²⁸ It is interesting that only these two possibilities are mentioned by Putnam, and that they are the only two that immediately spring to mind. Might there not have been a society, like Kripke’s society of quadders, or one in which the outcome of “ $2 + 2 =$ ” was always random? Or the outcome was not always a number, in which, for example, sometimes the appropriate answer was “nothing” or “banana”. Think about the fact that we *accept* something like “I don’t know” as an answer in certain situations, while not considering this to undermine someone’s understanding. Imagine a child given a particularly complex multiplication, say one involving fractions, they might answer with “I don’t know”, yet we might still be prepared to allow that they have mastered the technique of multiplication, if only for whole numbers. Indeed, we would usually attempt to teach them how to answer the new problem *on the basis of* such an assumption.

Putnam mentions. I have quoted all three because I think it is important to note their full import, but this will become clearer in the next section and after we have seen why Putnam does not think this other “Wittgensteinian move” is acceptable.

Putnam goes on to say of the sort of “move” outlined above that it “depends heavily on overlooking or denying the circumstance that an empirical fact can have a partly mathematical *explanation*” (Putnam 1979 p. 428). He then goes on to give some examples of circumstances in which he believes that apparently entirely empirical facts could have such partly mathematical explanations. He mentions three examples; a Turing Machine that is run for two weeks on a certain input and does not halt, the construction of a proof by human beings over many generations that Peano Arithmetic is consistent to 10^{20} places, and an experiment where on “five thousand occasions two things are added to two things (using some physical operation of combination) and the resulting group is counted” (Putnam 1979 p. 428). Each of these examples has particular problems of its own. I want only to mention what they are and then to return to the quote from *Philosophical Investigations* above to make a general point as regards Putnam’s use of all of them.

The import of the first example should seem familiar to us. This type of example is fairly common in commentary on Wittgenstein’s views about such things. We have seen a similar point put forward by Michael Dummett in his paper “Wittgenstein’s Philosophy of Mathematics”, and we have already seen the problem with examples of this kind (Dummett 1966 p. 428). The point is that to use such a machine analogy requires having a very particular conception of a machine. Putnam is of course honest about this, he describes the empirical fact as a describing a circumstance where what is involved is a “Turing Machine”, but nonetheless the specter of disanalogy still lurks in the background. What seems dubious here is the claim that what is being described *could* in fact be a circumstance that *would* actually be one that was described by an empirical fact at all. A Turing Machine is a conceptual entity, it is not a materially existent thing. There may be physical machines that are physical *instantiations* of a Turing Machine, but they are not

Turing Machines. Putnam claims that the fact that some actual machine that *instantiated* a Turing Machine did not halt is to be given a *mathematical explanation*. I would argue that this is only true *in so far as it is considered to instantiate that Turing Machine*; that is, in so far as what the actual machine does is considered from a symbolic perspective. In so far as the performance of the machine is seen as the performance of a Turing Machine and not an actual machine the explanation of why it did not halt *will* be a mathematical explanation. And as the previous discussion of this type of example should have made clear this explanation will, in some sense, already be part of the concept of the machine itself when conceived mathematically. In so far as the performance of the actual machine is seen as the performance of *an actual* machine the explanation will, naturally enough, be mechanical. (PI §193-4; RFM, III, §87, IV, §48, VI, §39)

Now, let me not be misunderstood. I am *not* denying that the actual performance of the machine—even if it doesn't instantiate a Turing Machine—cannot *give rise* to a mathematical explanation of that performance. To deny this would be to view things in a very queer way. The actual performance of the machine may give rise to a mathematical explanation, but only in so far as the actual performance is *modeled* on a symbolic or mathematical representation of the machine. And as stated, this model would already implicitly contain the mathematical explanation of why the machine did not halt. The mathematical explanation is thus not part of the empirical one, rather it lies along side it or some such. Putnam is correct in that we would ordinarily explain why the actual machine did not halt with a mixture of the two explanations, but strictly speaking they are distinct. And the mathematical explanation can only explain why the mathematically modeled machine did not halt, because when the machine is mathematically modeled the reason it cannot halt is the reason the mathematical explanation gives us. That is, a mathematically modeled (Turning) machine cannot halt on the input...because it is not so conceived to.

Secondly, we have Putnam's example of a proof of the consistency of Peano Arithmetic to 10^{20} places being given over several generations. Putnam says that *if* such

proof could be provided “is not the *explanation* of this fact simply that, as a matter of *mathematical* fact, Peano Arithmetic is 10^{20} consistent, *and* the human beings took sufficient care so that the putative proofs they examined during the long search really *were* proofs in Peano Arithmetic?” (Putnam 1979 p. 428). This is, again to some extent, a matter of perspective. With hindsight we might want to agree with Putnam that *so far* Peano Arithmetic has not turned out to be inconsistent. And once the proof has been abstracted from the activities involved in giving it, we might want to say that we would not reject it. But during the “long search” as he calls it “sufficient care” has to be taken. How are we to understand what is meant by “sufficient care” here? Couldn’t we respond to this by saying that “sufficient care” is best understood here as making sure that those who were deriving the proof stayed within the “framework on which the working of our language is based” (PI §240)? And isn’t this a matter of making sure that those deriving the proof make the same presuppositions as the rest of us do?

Again, I am not trying to say that this proof of the 10^{20} consistency of Peano Arithmetic is not a valid proof, or that a mathematical explanation could not *arise* from the historical facts as they stand, for example that mathematicians never came to blows over the question of whether the rules had been followed correctly. The point is the mathematical explanation is not the same as—does not explain the same thing as—the historical one. Although the mathematical explanation, on this story, could not be given until the historical one had been given. Or at least, until the history of the “long search” had been told. So we could still say that it *was* a matter of empirical fact that there were no disputes, and that *given* this empirical fact we say that we can now view the situation *with hindsight and mathematically* and say that given those historical empirical facts Peano Arithmetic is 10^{20} consistent, and this is *not*, at least on the face of it, a mathematical fact.

Finally, Putnam describes another experiment in which the addition “ $2 + 2 = 4$ ” is performed five thousand times using some “physical operation of combination”; in two

hundred of the operations the result is not “4”, and apparently on investigation we find that a member was either added or taken away from the group during these situations. Putnam then asks us:

Is not the explanation of the fact that in the remaining 4,800 cases the result of the count was “4” just the fact that in *those* cases no individual involved in the combining process was destroyed or otherwise removed from the group counted at the end; that no individuals were added to the final group by any interactions; and that, as a matter of simple arithmetic fact, $2 + 2 = 4$? (Putnam 1979 p. 428)

He goes on to ask “how it is that we actually *found* (as opposed to just *positing*) an explanation of what went wrong in the deviant 200 cases?” (Putnam 1979 pp. 428-9). The point here seems to be that it is just a fact about the way the world is that we *will* be able to find explanations for the deviant two hundred cases. That is, it is just a mathematical fact that if the result of an addition, performed by way of a “physical operation of combination”, yields a result that is not consistent, then we will be able to, and should look to, explain what went wrong in terms of some individual being added to or removed from the group. He says of the possible response that “it is just a *surd* empirical fact that one *does* find such explanations in such cases”, that we would seem to be abandoning “the whole world view of science since Newton for a very strange metaphysics” (Putnam 1979 p. 249).

The point is, according to Putnam, that the empirical fact that we did actually find explanations for the two hundred deviant cases is apparently not enough, we need to add the further fact that the explanations were there to be found, and this further fact is a *mathematical* fact, it is the fact that in calculation the outcome described by “ $2 + 2 \neq 4$ ” is not acceptable. And this is backed-up by the empirical fact that physical objects just don't combine in ways that would agree with what “ $2 + 2 \neq 4$ ” implies. Hence Putnam's claim that to reject the mathematical fact as part of the explanation of the experimental results is to impugn the scientific world view, for a strange alternative one, and as Putnam puts it:

...on the alternative metaphysical picture, there are just all these surd empirical facts *and* the way we *talk* about them. We do not often come up with apparent counterexamples to " $2 + 2 = 4$," but it is not *because* two and two *do* make four that we do not. Rather, on the picture just suggested, it is *because* we do not often come up with apparent counterexamples that we *say* " $2 + 2 = 4$." Why should anyone believe *this*? (Putnam 1979 p. 429)

I think that the answer to this series of questions, and in particular to the last, is the more general answer drawn from §'s 240-2 of *Philosophical Investigations*. As I have said, I think that this answer has great import for all three of the above examples, but I think we will need a new section to do justice to the full import of these three passages.

§ 11. My View, part I: The Constitution of Meaning.

In sketching out the full import of the three passages from *Philosophical Investigations* quoted above we start with §241:

"So you are saying that human agreement decides what is true and what is false?"—It is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions but in form of life.

Understanding the full import of this passage is the key to understanding the full import of the others, and in my opinion much of Wittgenstein's later philosophy, particularly as regards what we should say about the meaning of logically necessary statements. The important thing to notice here is the rejection of the suggestion that "human agreement decides what is true and what is false", what is being rejected here is the idea that the agreement is on what is true and what is false. The agreement is rather agreement in the language used, and "[t]hat is not agreement in opinions but in form of life". So what is in question here is not whether we agree in how we see the world, what we think it is like, but rather whether we agree in how we communicate about it. I take this to be an explicit rejection of the extreme constructivism that Putnam seems to want to attribute to Wittgenstein, but I also consider it to open a possible route to reading him as not challenging the objectivity of mathematics.

I mean this in the following way: the objectivity of mathematics is actually a matter of which statements in mathematics, or in logic for that matter, we say are true and false. We say that the statements " $2 + 2 = 4$ ", "Nothing is at the same time red and green all over", and "998, 1000, 1002, is going on in the same way as 2, 4, 6...", are all objectively true. And what this seems to mean is that we say that their truthfulness is not a matter of its being recognized by us. As I said there are at least two notions of objectivity; a notion belonging to metaphysics, and a notion belonging to the theory of meaning. The later notion is the notion that we as subjective beings can come to grasp the apparently objective meaning of mathematical statements, and what *this* means is that we come to participate in a shared form of life by saying along with others that, for example, the two mathematical statements mentioned above, are true. We have grasped the objective meaning when we say that the same mathematical statements are true as others do, and we do this when we come to agree with them in the language they use. We cannot be said to agree with them in the language they use if the majority of the foundational statements they say are true are not also foundational statements we would say are true.²⁹

It is at this point that we should turn our attention to §242.

If language is to be a means of communication there must be agreement not only in definitions but also (queer as it may sound) in judgments. This seems to abolish logic, but does not do so.—It is one thing to describe methods of measurement, and another to obtain and state results of measurement. But what we call “measuring” is partly determined by a certain constancy in results of measurement.

The first line of this quote rephrases what I have just been trying to say, that for "language to be a means of communication there must be agreement...in judgments", that means we must most of the time say of the same foundational statements that they are

²⁹ I am not using “foundational” in an epistemological sense here. Foundational statements are those statements that form the framework or stage-setting for such activities as calculating, counting, and so on. These sorts of statements are often expressed by Moore-type propositions like “here is a hand”, and are the sorts of propositions Wittgenstein describes as riverbed or bedrock propositions in *On Certainty*, and whose negations he held to be nonsense, because, apart from other things, to affirm their negations seems to involve ceasing to communicate at all (OC §37, §§95-9, §213, §248, §461 & §657).

true. The relevance of the comparison to measuring should be obvious, "what we call 'measuring' is partly determined by a certain constancy of results", without this "constancy of results of measurement" we would not have a practice anything like the one we call "measuring". And if that practice lost the constancy of results that it currently has, then we would be unlikely to call it "measuring" anymore. And even if, perversely, we did, we would still have to agree about the truth of many of the statements that we have about measurement; but in the absence of a constancy of results there would be room for disagreement about the truth of these statements. I believe that the scope of such disagreement would be enough to make this a situation in which it would be incorrect to say we agreed in the language we used, that there was agreement in form of life, or at least that measuring language could any longer be a form of communication. To see this more clearly cast your mind back to the woodsellers scenario and how they price and sell wood. (We will discuss the statement about the apparent abolition of logic in a moment.)

This brings us to the first passage of the three: §240.

Disputes do not break out (among mathematicians, say) over the question whether a rule has been obeyed or not. People do not come to blows over it, for example. That is part of the framework on which the working of our language is based (for example, in giving descriptions).

This passage backs-up the claim in the foregoing two with the observation that the sort of disagreement which a lack of constancy of results in measurement would induce does not occur. For the practice of measuring to be what it is there cannot be such widespread disagreement about which statements of measurement are true and which are false. The case is just the same for mathematics, if mathematicians could "come to blows" over whether or not "a rule has been obeyed or not" in giving a proof for example, then you could not have a practice anything like the ones we call "calculating", "counting", and so forth. These practices form a family, and the main trait they share is that there is *not* in fact disagreement about which statements associated with them are true and which false. This is the "framework on which the working of our language is based" a framework of

agreement in practices that makes for our form of life, for example agreement in the practice of giving descriptions.

We can now return to the sentence in §242, that agreement in judgments "seems to abolish logic, but [that] it does not do so". I take this to be an indirect reference to what we have been referring to as radical conventionalism. If we understand "logic" here to be referring to logical inference in general, then it becomes clear that the need for agreement in judgments could abolish logic in just the way that we might say radical conventionalism does. Radical conventionalism, as Dummett has eloquently shown, abrogates logical inference in so far as it substitutes *decision* for *recognition*, in so far as it says that we *choose* whether or not to accept a conclusion as following from some set of premises. That is, if we are properly described as needing to agree "not only in definitions...but also in judgments" then it seems like we could have defined our logical connectives axiomatically, and still be in the position to *choose* whether to endorse something as the legitimate conclusion of a chain of inferences even though given *our own* definitions *as we meant them*, this is not something we ought to be in a position to make a choice about. Now, we need to return to §241 to see why this does not abolish logic, or in our terms logical inference.

Section 241 draws attention to the fact that we agree in the language we use, in our form of life, and this is not the same thing as saying that it is our agreement that decides what is true and what is false. The need for agreement in judgments could only abolish logic if such agreement in judgments *did* decide what is true and what is false, but it *does not*, what such agreement *does* decide is which statements we *say* are "true" and which we *say* are "false". We are in agreement in judgment in *saying* that foundational statements like "Nothing is at the same time both red and green all over." or " $2 + 2 = 4$ " are true, and even that they are "logically true", and such agreement *is agreement in the*

language we use. What this means is that logic is only abolished if such agreement in the language we use could be in *disagreement* with, say, our agreement in definitions.³⁰

Again, we need here to return to §240, and the observation that "[d]isputes do not break out...over the question whether a rule has been obeyed or not...[t]his is part of the framework on which the working of our language is based". The point is that we could not have agreement in the language we use unless we have this framework in which disputes do not break out in most areas about whether a rule has been correctly followed. And even where such disputes are legitimately conducted it is only because there are many other rules, say for example, about how to conduct the dispute itself,—parts of the framework for the working of our language—for which the question of whether they are being correctly followed is never raised. In §224 and §225 of *Philosophical Investigations*, which also address this theme Wittgenstein tells us that:

The word "agreement" and the word "rule" are *related* to one another, they are cousins. If I teach anyone the use of the one word, he learns the use of the other with it.

The use of the word "rule" and the use of the word "same" are interwoven. (As are the use of "proposition" and the use of "true".)

And I think that these passages make clear the sense in which agreement in the language we use can at the same time be agreement in judgments while not abolishing logic.

Agreement, in the sense in which it is used in "agreement in judgments" does not abolish

³⁰ To clarify what is meant here by "agreement in definitions". Ben-Menahem sees conventionalism as a modification of the traditional "laws of thought" view often attributed to Immanuel Kant among others (Ben-Menahem 1998 pp. 102-3). She tells us the following about conventionalism.

Conventionalists...[point]...to two links between the necessary and the conventional. First, there is a certain phenomenological similarity between speakers' treatments of so-called necessary truths and their treatments of deeply-entrenched rules...The second more important link is supplied by the notion of *constitution*. Conventions are often constitutive of social activities and institutions...The conventionalist sees necessary truths as playing a similar role in the realm of thought: they are constitutive of our forms of reasoning...Unlike laws of nature, which purport to describe an external reality, the laws of thought are constitutive definitions. (Ben-Menahem 1998 p. 104)

I think that, at least in so far as Wittgenstein's conventionalist is concerned Ben-Menahem is correct, and I think that what she says here, also throws light on why Wittgenstein would have said that there needs to be agreement "not only in definitions...but also in judgements" for such agreement is itself what constitutes the core of communication.

logic because it *is* logic. Logical inference is a matter of following rules, and we are told above that the words "rule" and "agreement" are related. The sense in which these two are "cousins" is the sense in which agreement in language is the framework of rules on which the working of our language is based. To say that there is "agreement in judgments" is to use "agreement" in a very basic sense of the word; it is the sense in which agreement just is part of following the rule, and "the use of 'rule' and the use of the word 'same' are interwoven", our agreement in judgments is in this sense of "agreement" our going on in the same way in following the rule. I am of the opinion that the view which is emerging here has significant implications for what has been said so far by all of our authors about Wittgenstein's view of logical necessity.

As I said earlier in a footnote, Ben-Menahem makes the distinction between "philosophical" and "innocent" uses of the terms "justification" and "explanation" and I think that similar remarks might apply to Wittgenstein's use of "agreement" here. She says that for Wittgenstein those situations in which we would properly describe ourselves as, for example, appealing to the rules of inference to *justify* some chain of reasoning, or in *explaining* why one must go in one way rather than another, these terms are being used "innocently".

Admittedly, on some occasions one may wish to employ the notion of rule or convention to either justify or explain behavior...Wittgenstein does not fault such ordinary exchanges, but rejects the philosophical theory that depicts rules as abstract entities that predetermine their applications, or 'cover' them as a natural law covers its instances...We do in fact justify particular moves by showing how they follow from rules, indeed, this is precisely what we call justification. But in a deeper sense rules cannot justify their applications, they simply belong together as do pieces of a linguistic puzzle... (Ben-Menahem 1998 pp. 115-7)

I take it that what she means here is that while we might *describe* what we do here as "explaining" or "justifying" these words are not being used in the way the philosopher would use them. Wittgenstein's descriptive conventionalism holds that in such situations

we are properly described as "explaining" or "justifying" the move in question *only to the extent that what is involved here is an ordinary language exchange*. As soon as this exchange takes on a deeper philosophical dimension the description of us as explaining or justifying ceases to hold up as a result of the rule-following paradox: "no course of action could be determined by the rule, because every course of action can be made out to accord with the rule" (PI §201; Ben-Menahem 1998 pp. 114-7).

In my view, similar remarks apply to the way "agreement" has been used above. Wittgenstein's use of "agreement" is philosophically innocent in the same way that certain notions of justification and explanation are. Wittgenstein says, "[i]t is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions but in form of life" (PI §241). And as I have said earlier, and as Stroud has stressed, the way in which the notion of agreement is being used here is *not* a way in which one can properly be said to understand its negation, that is, to understand the idea that there could be such a thing as disagreement at this level (Stroud 1966 p. 491). *If* that *was* the sense in which "agreement" was being used in the above quote, *then* Wittgenstein would have answered the question, "[s]o you are saying that human agreement decides what is true and what is false?" in the affirmative; but this is exactly what he *does not* do (PI §241).

§ 12. My view part II: What Things Mean to Us.

The view which emerges from these considerations can perhaps be baldly stated by saying that the fact that meanings of our statements are constituted by convention does not make it a fact that *what they mean to us* is conventional. At the end of his paper "Wittgenstein and Logical Necessity" Stroud says the following about Wittgenstein and logical necessity.

Logical necessity, he says, is not like rails that stretch to infinity and compel us always to go in one and only one way; but neither is it the case that we are not compelled at all. Rather, there are rails we have already traveled, and we

can extend them beyond the present point only by depending on those that already exist. In order for the rails to be navigable they must be extended in smooth and natural ways; how they are to be continued is to that extent determined by the route of those rails which are already there. I have been primarily concerned to explain the sense in which we are "responsible" for the ways in which the rails are extended, without destroying anything that could properly be called their objectivity. (Stroud 1966 p. 496)

This passage follows on almost immediately from a quote he takes from Dummett's paper "Wittgenstein's Philosophy of Mathematics" in which Dummett proposes an alternative view to the radical conventionalism he attributes to Wittgenstein which he claims lies "between the Platonist and constructivist pictures of thought and reality" (Stroud 1966 p. 496).

an intermediate picture...of objects springing into being in response to our probing. We do not make the objects but must accept them (this corresponds to the proof imposing itself on us); but they were not already there for our statements to be true or false of before we carried out the investigations which brought them into being. (Dummett 1966 p. 496)

We should remember that this is a *metaphysical* picture, not one about meaning, it is a picture about how the world comes to us, not about what our statements mean. The view described here is very much like the cautious constructivism that Putnam gestures toward when he points out that the rule following considerations *do not* show that the consistency of Peano Arithmetic *must* be an artifact of the way we use the words (Putnam 1979 p. 427).

Earlier on I made the following points; that I thought that the shift to constructivism from conventionalism originates with Stroud's introduction of the notion of form of life. That Putnam attributes an extreme constructivism to Wittgenstein and shows that extreme constructivism *must be untrue*. And finally that this is different from the point that radical conventionalism is *logically incoherent*., that although the two criticisms are related, they are not the *same*. Taking these points as premises I think we can draw the sound

conclusion that if Wittgenstein was an extreme constructivist he was definitely wrong, but that this would also mean that if he was also a radical conventionalist that his account was not logically incoherent. In other words, opting for an extreme constructivism builds in absolute consistency and so does not make radical conventionalism about the meanings of logical statements an acceptable option.

Let me be clear. Extreme constructivism has, so far for us, been represented by the idea that Peano Arithmetic must be an artifact of the way we use words. The idea here is that extreme constructivism implies the absolute consistency of Peano Arithmetic in Putnam's sense. But if this were so, then it hardly makes any sense to say that one could at the same time be an extreme constructivist and a radical conventionalist, since to be a radical conventionalist one must hold that it is possible for us to reach a conclusion in, say, Peano Arithmetic which *is* inconsistent with Peano's Axioms, and the rest of Peano Arithmetic. The radical conventionalist after all, holds that it is a matter of decision for us as to what counts as going on in the same way—as to what can be drawn as the conclusion of a chain of inferences from an already agreed to set of premises and inference rules—this view is, in my opinion appropriately, *radically incoherent*. The only way that one could *appear* to be a radical conventionalist would be if one did in fact hold to an extreme constructivism, but then, since the way the world is *is* a matter of the way we use the words, our *deciding* how to go on could come to no more than our all agreeing in how we use the words; it would just be a happy coincidence that we all decided to go on in the same way at the same time.³¹ The difference between this sort of agreement about how we use the words, and what I take to be Wittgenstein's notion of agreement

³¹ Ben-Menahem describes radical conventionalism with the following contrast:

The modified conventionalist distinguishes between basic conventions, directly agreed upon by the community, and consequences of these conventions, the truth values of which follow from the basic conventions. By contrast, for the full-blown conventionalist, each individual grammatical rule, and, moreover, each application of a grammatical rule constitutes an expression of a new convention. Necessary truth is a matter of human choice on both versions of conventionalism, but whereas the modified conventionalist grants the community a certain privileged status by virtue of its authority to stipulate basic conventions, the full-blown conventionalist grants each individual unrestricted freedom to stipulate a necessary truth with each pronouncement (Ben-Menahem 1998 p. 100).

about how we use the words, is that this use of "agreement" is not "philosophically innocent" in Ben-Menahem's sense. The extreme constructivists consider themselves to be telling us something *sophisticated* about the way the world is (Ben-Menahem 1998 p. 115-7). And, to my mind more importantly, they are making a *metaphysical* assertion. In contrast to this Wittgenstein gives us the following in *Philosophical Investigations*.

218. Whence comes the idea that the beginning of a series is a visible section of rails invisibly laid to infinity? Well, we might imagine rails instead of a rule. And infinitely long rails correspond to the unlimited application of a rule.

219. "All the steps are already taken" means: I no longer have any choice. The rule, once stamped with a particular meaning, traces the lines along which it is to be followed through the whole of space.—But if something of this sort really were the case, how would it help?

No; my description only made sense if it was to be understood symbolically.—I should have said: *This is how it strikes me*.

When I obey a rule, I do not choose.

I obey the rule *blindly*.

As I have been trying to stress. Wittgenstein's concern lies in the area of the philosophy of meaning, not the philosophy of mathematics or metaphysics. From the point of view of the philosophy of meaning, the fact that we agree in the way that we use the words is just a brute fact about the framework on which the working of our language is based, this fact implies nothing about the way the world is or has to be. Wittgenstein points out on page 230 of part II of *Philosophical Investigations*, that the world might have been a very different place and so we *might* have realized very different concepts, but we *might* just as well have ended up realizing the same ones for all that, because the way a statement gets its meaning, does not show what it means to us.

I am not saying: if such-and-such facts of nature were different people would have different concepts (in the sense of a hypothesis). But; if anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean not realizing something that we realize—then let him imagine certain very general facts of nature to be different from what we are

used to, and the formation of concepts different from the usual ones will become intelligible to him. (PI II p. 230)

I have been concerned in the earlier part of this section to show that Wittgenstein did not hold to radical conventionalism about the meanings of logical statements, that the sense in which for language to be a means of communication there must be agreement in judgments as well as in definitions is a very basic sense of "agreement" because the use of that word and the use of the word "rule" *are* so closely related. The framework on which the working of our language rests is a framework of rules with the use of "rule" and "same" being interwoven. Learning how to follow a rule is learning how to go on in the same way, and it is also to learn what it is to be in agreement. Logical inference, and hence logical truth and logical necessity, are not abolished by these considerations, because logical inference gets its whole being from which logical statements we say are true and which we say are false. This is the sense in which the use of the word "proposition" and that of the word "true" are interwoven (PI §225). But our *saying* so is not what makes them true or false, though we say *of* them that they are true or false, this is not something we *decide*. In a sense this decision is made for us by those agreements which make communication possible, by our agreement in form of life.

Conventionalism is true, and Wittgenstein was a conventionalist, just in so far as it is true that we agree in the language we use, in so far as we subscribe to the same conventions. While conventions can constitute meaning, and hence conventionalism can tell us how meaning comes about, it cannot tell us what those statements whose meaning is so constituted *mean to us*. To find this out we have to know what sort of role those statements play in our form of life. This is not the sort of thing you can *explain* it is something you can only *describe*, and hence conventionalism cannot tell us *why* we agree in our judgments, but only *how* agreeing in judgments can constitute the meaning of, say, logical statements.

It might perhaps help to briefly consider the following tale. A man visits the Japanese quarter in his local city. He observes that almost all verbal exchanges are accompanied by a slight nod of the head. Thus far he has everything he needs to realize that the nod of the head gets its meaning by way of the conventional character of the gesture. All the same he will not be able to realize what the nod of the head *means to the Japanese*. Indeed, it might mean nothing to him initially, it might just be the convention, but if he spends long enough in with the Japanese—long enough to become a participant in their form of life—then he will probably realize at some point what the nod of the head *means to us*; which will of course be the appropriate way to put it because he is now a participant in that form of life. All the same the Japanese will never be able to explain to him what the nod of the head means to them, all they can do is to point to the convention, the agreement between them about the appropriate uses of the gesture. We are like the Japanese, we cannot point to anything other than convention about when one should and should not do whatever; we can *describe* our conventions about logical inference to an outsider, but we could not *explain* them: indeed we cannot explain them to ourselves without falling into confusion. And this is Wittgenstein's whole point, we can't and we don't need to.

I said at the beginning of this chapter that I thought that the roots of this dilemma laid within the initial conception of conventionalism favored by the logical positivists, and that Putnam rightly thinks that this view is weird. Hopefully, this remark makes sense now. The logical positivist conception of convention and conventionalism collapses the idea that the meanings of our statements are constituted by conventions with the idea that what they mean to us is merely a matter of convention, and this conception leads directly, as Wittgenstein shows us, to radical conventionalism. The first part of the positivist conception is correct, the meanings of our statements are constituted by convention, the second part is incorrect, what those statements mean to us is *not* merely a matter of convention. An exhaustive list of all the conventions we subscribe to would not show us what those conventions mean to us, to show this you need to incorporate them into a form of life. A form of life is not something that can be explained, it can only be described, and

the only way to do so is to point to all the surd empirical facts about us that make us who we are. As such, metaphysically speaking, the moderate constructivism that Stroud, Dummett, and Putnam, all seem to be in agreement on, is probably the best we will ever get. But acceptance of this view at the metaphysical level is only related to, and does not entail, acceptance of a conventionalism like Wittgenstein's in the theory of meaning, *because metaphysics is a matter of what the statements mean to us, not a matter of how they get their meaning*

Conclusion

I said in my introduction that I would attempt to achieve five things with this thesis, and it is now time to see if I have done that. In the first place we have my claim that the issues considered in this debate belong more properly to the philosophy of meaning. I take myself to have adduced strong grounds for this claim in particular. Aside from the fact that much of what has been said in this debate becomes much easier to understand when the issues are seen this way, I believe that Stroud's grounds for rejecting the attribution of radical conventionalism to Wittgenstein also support this claim.

The central question which all accounts of logical necessity have sought to answer is on my view in fact divisible into two sub-questions. And I believe these questions can and indeed *must* be answered separately and independently. The central question might be posed as: (Q) what is it about our grasp of the meaning of a logical truth that commands our acceptance of it? This question can be recast—and I believe it needs to be—as two sub-questions. (Q: 1) How do logical truths get to have any objective meaning at all? (Q: 2) What do they mean to us such that they can command our assent? It seems to me that close examination of past attempts to answer the central question shows that they usually only manage to answer one part of it, in general (Q: 1). Both Platonism and modified conventionalism provide convincing answers to (Q: 1). The Platonist says that it is just a feature of the sorts of things that logical truths are; the conventionalist that it is a matter of intersubjective convention. That is, these views answer the (Q: 1)—How do logical truths get to have any objective meaning?—with a description, on the one hand of some part of reality, and on the other of our practices as a group. The problem comes when these views try to stretch their answers to cover (Q: 2)—what do they mean to us?—by trying to answer (Q). At this point the notion of understanding has to be brought in to turn what have so far been *descriptions* of logical truths into *explanations* of them.

As regards my second goal. Stroud is mainly concerned to show how for Wittgenstein our being “responsible” for the creation of logical and mathematical truths, as a conventionalist would hold we are, does not involve “destroying anything that could properly be called their objectivity” (Stroud 1966 p. 496). Stroud’s own strategy of argument has been to show that we can read Wittgenstein’s examples as challenging traditional Platonist and conventionalist accounts of logical necessity, *and* as ultimately unintelligible.³² He takes this to imply that Wittgenstein was trying to draw our attention to the central weakness in these traditional accounts—the particular conception of understanding they draw upon—rather than providing an alternative account himself. I have sought to flesh out this claim in more detail, and to embellish some samples of those examples in the way that Stroud suggests. And I have argued that the two types of examples are actually deployed by Wittgenstein in order to develop the logical point that logic cannot arbitrate as to what actually counts as logical and what does not. This logical point itself constitutes the argument for my earlier assertion that (Q: 1) and (Q: 2) *must* be answered independently. To answer (Q: 1), as the traditional accounts do, with a description, in no way provides us with any guidance as to *what ought or ought not be counted as logical*, it simply describes what we *all ready regard* as logical.³³

Third, we have the rejection of radical conventionalism on two fronts; as the view that Wittgenstein held, and as being a plausible alternative whether he held to it or not. The second rejection, which probably actually amounts to a refutation, is not one that I have much to say about. On this I am in agreement with what Dummett has to say, as is Stroud. All the same, the above points about Platonism and conventionalism still apply to

³² It is interesting that when these seemingly plausible counter-examples are pressed they seem to dissolve under the weight of unintelligibility. Did Wittgenstein intend this to happen? Did he deliberately frame such examples as heuristics which don’t actually provide a solid basis for ascribing any alternative account to him?

³³ It might sound strange to claim that Platonism gives us a “description” of what we regard as logical, but I think that this is the correct way to put things. The Platonist can only point to those logical truths we have already discovered, how we are to discover new logical truths is still a mystery. We can compare the situation to one in which we are trying to find say, all the triangles in a puzzle-picture. For the Platonist the triangles are no doubt there, but having spotted some doesn’t mean we will be able to spot all the others.

the radical species of conventionalism. Radical conventionalism doesn't really get us any further, it cannot, like its predecessors answer (Q: 2) in any informative way. To say that we all feel like choosing how to go on in the usual way when continuing the series is not an answer to (Q: 2). In so far as the rejection of Dummett's attribution of this view to Wittgenstein goes, I think that again I let Stroud do most of the work of argument. Although I think that my embellishment of the examples, and my elucidation of the logical point they develop, support this rejection, *and* provide a clear basis for the attribution of some sort of less radical conventionalism to Wittgenstein.

The argument against the claim that Wittgenstein was an extreme constructivist, my fourth goal, has much more of what I have to say in it. This argument, again, draws heavily upon Stroud's conclusions about the examples and his exposition of them. But there are some respects in which what Stroud says is not enough. Putnam has proposed a radical shift in the arena of debate, and to my mind an unjustified one. While I have argued that the roots of such a shift might be found in Stroud's side-stepping of the radical conventionalist attribution, I think that it is Putnam who really stakes out the new ground. I believe that Putnam has actually tried to suggest his own alternative to both traditional (Platonist and conventionalist) and more modern (Wittgenstein's and Quine's) attempts to answer what I have called the "original question" about logical necessity. Putnam has a new strategy, but I don't believe it works. To my mind Putnam attempts to answer (Q: 2) first, and then on the basis of that to go on to answer (Q: 1). Thus he endorses the metaphysics given us by modern science, and makes an appeal to objective mathematical facts and the role of mathematical explanation in empirical explanation. Not only this, but as Dummett's response to Putnam—which I do not have anything to say about in this thesis—makes clear, this new strategy would turn what was previously a debate about meaning into a debate about metaphysics, or more precisely about internalism and externalism.

Finally, we have my endorsement of Ben-Menahem's attribution of descriptive conventionalism to Wittgenstein. Just to be clear, I take descriptive conventionalists to hold that: the formation of logical truths, consistency in logical reasoning, and extension of logical or mathematical sets, are all matters of convention. They say that to describe what we do in reasoning one should point to the conventions we all share as being what give meaning to the steps, or statements, premises, or whatever, taken when reasoning in logical and mathematical ways. As such logical necessity is said to be a matter of convention. Logical compulsion is not explained on this account, and by my lights it would not be. Why we feel compelled to go on in this way rather than that will be a matter of what the statements, axioms, proofs, and rules, of logic and mathematics mean to us, not a matter of how they get their meaning. Which, as I have said, is to be *described* as a matter of convention. This conception differs from Ben-Menahem's in a few ways, particularly in its implications.

Most importantly I do not follow Ben-Menahem in reading Wittgenstein as a realist. I do not hold either that he did in fact give us a complete or even mostly-complete basis for an anti-realist semantics, as Dummett seems to think he did. But I do believe that he is moving in the direction of anti-realism; at the moment I still hold out hope for a more sophisticated and subtle anti-realist account that would do justice to him. Secondly, I do not think, as Ben-Menahem does, that Wittgenstein *ever got to* holism about meaning, though I would not deny that he was moving toward it. I believe that there might be a good reason he stopped where he did, holism about meaning seems suspect to me, it seems to presume too much knowledge on the part of speakers. I think that a more nebulous approach based around Wittgenstein's notion of language-games could be more successful, and more accurate (almost like a pluralism about meaning). Such an account would not be atomic, but it would build from mid-sized chunks of meaning, and I hold out hope that such an account could incorporate the work of another philosopher who I think was on to the same idea, J. L. Austin.

Finally, I think I should say something about where within the general scheme of philosophical discourse I believe my work ought to be placed. With this thesis I have sought to construct a body of research and argument that can form a support-structure for further investigation. I consider this thesis to be within the field of philosophical logic, but reaching out, as it were, to other areas such as the theory of meaning and philosophy of science. I want this body of research to form the basis of a further project within the philosophy of science. I regard what I have presented, and the immediate implications that might be drawn out from it, as forming the basis of a critique of a certain view about logic popular within so called “sociological” studies of science. But first we need a little history.

It is well known that under the pressure of internal critique and consistency worries, that the philosophy of science virtually imploded during the 1960’s, the effective causes of this implosion were the writings of philosophers like Thomas Kuhn and Paul Feyerabend. Wittgenstein’s writings, or at least allusions to them, figured significantly in this process of self-critique. As a result of these problems there was a general methodological shift away from standard analytic approaches. In particular the continental approach exemplified in the work of scholars like Michael Foucault became popular. The result was closer alliance between historians of the politics of science and philosophers of science who saw their proper subject matter of study as the social machinations that resulted in the production of scientific knowledge. Within this environment the general area of social studies of science blossomed. And, curiously enough, Wittgenstein’s writings again figured significantly, only now they were used to provide the material to rebuild the philosophy of science. Of course, this is not just the work of sociologists of science, the work of many philosophers contributed to this state of affairs. In particular, Saul Kripke’s contributions toward Wittgenstein scholarship.

Kripke’s reading of Wittgenstein, so controversial that it often gets labeled “Kripkenstein”, has caused a stir both within and without analytic philosophy. And it

seems to have provided inspiration for some philosophers of science. Kripke's reading has two main features which seem to have impressed these philosophers of science. In the first place he attributes a paradox to Wittgenstein which apparently motivates Wittgenstein's discussions of rule-following behaviour involving the two special cases of mathematics and psychology. That is, discussions about how one comes to follow rules that seem to command acceptance, and about how one comes to attribute psychological states to other persons and to oneself in a consistent way. Secondly, Kripke attributes a skeptical solution for this paradox to Wittgenstein in the form of the private language argument. The argument that our capacity to follow rules cannot be explained by the idea that there is a private language of sensation, ideas, or whatever, into which the public explanation of how we are to follow the rule has been translated when we come to understand it. As such all language is held to be public, and all rule-following is public, hence all rule-following is held to be constituted by the social practice of which it forms part. Any impression that there is an "inner" motivation, justification, or ground for following a rule is seen as a case of mistaking the accompaniments of the activity of following rules for that activity itself, and hence is a case of mistaken identity and confused nonsense philosophising.

Philosophers of science working within the area of social studies of science have taken to Kripkenstein with vivacity. Kripke's insightful interpretation has been seized upon and developed by authors like David Bloor, who has read Kripkenstein as advocating an interpretation of the later Wittgenstein which sees him as promoting a "sociological" approach to the theory of knowledge, and the analysis of language and linguistic meaning.³⁴ The keystone of this sociological interpretation is what Kripkenstein apparently has to say about rule-following behaviour involving psychology and mathematics. Kripkenstein is seen as giving us sociological analyses of the two hard cases of psychological predicates and mathematical and logical necessity. (Kripke does

³⁴ For this see Bloor 1983 and 1997.

not seem to endorse this interpretation of his work himself, and I think that a careful reading of his work would bear out his rejection of such an interpretation.) In my view such an interpretation unfortunately lumps both the good and the bad of modern day Wittgenstein interpretation in together, in short I think such an approach is sloppy. I see my own work in this thesis as providing some basic groundwork for a more thorough examination of Wittgenstein on the hard case of mathematical and logical necessity by showing in what sense his treatment might actually be regarded as sociological, or better still as having a sociological dimension.

I think there is no doubt that Wittgenstein has provided us with a range of exceedingly useful tools for the analysis of the history of science and science practice, which is what I understand the philosophy of science to do. But, to read him as promoting a broadly sociological approach misses the most important point: it imputes a theory to him when it was clear he was at pains to show he did not hold one. Again, there are no doubt some very useful insights to be gained from what sociologists of science like Bloor have to say about Wittgenstein—there are definitely some aspects of his philosophy that only show up under a sociological reading. I think that good Wittgenstein scholarship could make a very important contribution to the current revitalization of philosophy of science, which is well overdue. We just need to approach the task cautiously. We will *not* be coming up with Wittgenstein's philosophy of science, we won't be coming up with Wittgenstein's philosophy of anything. But we will be deploying his own tools of analysis—language-games, family resemblance, symptoms and criteria, form of life—in a way that allows us to reap the benefits of insight and perspicuous representation, and to avoid the pitfall of excessive abstraction from concrete cases to generalisations at the meta-level that are uninformative and unhelpful.

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